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ON THE ORIGIN OF BINARY STARS

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1. One of the fundamental problems of cosmogony may be stated as follows: Will a rotating mass of fluid, in equilibrium under its own gravitation, and free from sensible external disturbance, but subject to loss of heat and consequent contraction, eventually break up into separate parts; and if so, how will the separation take place? The only case which has been subjected to detailed mathematical investigation is the classical one of a homogeneous incompressible fluid. The general results (associated with the names of Maclaurin, Jacobi, Poincaré, and Darwin) are widely known. The originally spheroidal mass becomes more and more flattened, tends to lose stability, and recovers it by changing into an ellipsoid of three unequal axes, which similarly goes over into an elongated "pear-shaped" figure with one end larger than the other. According to Darwin, this last is also stable;[1] but at this stage the analysis becomes too complicated to pursue further. Darwin has, however, shown[2] that two masses of similar fluid cannot be brought close enough to coalesce without previously becoming unstable, and hence that if fission of a single mass occurs, it must be accompanied by a "period of turbulence." It is not, however, definitely known what would be the ratio of the resultant masses, or even into how many pieces the original mass would divide.

[1] *Phil. Trans.*, A, **200**, 251–314, 1903.

[2] *Ibid.*, **206**, 161–248, 1906.

But the actual stellar or nebulous masses with which astronomy must deal are gaseous—highly compressible, and much condensed toward their centers. The general mathematical investigation of the figures of equilibrium and their stability would probably in this case be very difficult. It is known that central condensation diminishes the ellipticity of the spheroid when the angular velocity is small, and very probable that it increases its stability.

On the other hand, Jeans has shown[1] that the compressibility of the gas tends toward instability, which may set in for relatively small angular velocity, and is of such a nature that the centers of the inner surfaces of equal density move away from the center of gravity in one direction, and those of the outer layers move in the opposite direction; while later these surfaces themselves become pear-shaped. The later stages of this process have not been followed mathematically. The detailed results (such as the exact rate of rotation at which a given change of form or stability occurs) would depend to an unknown extent upon the assumed law of distribution of temperature and density within the gaseous mass.

There remains therefore a good deal of uncertainty as to what would actually happen to a rotating and contracting mass of gas.

Sir George Darwin, in a recent essay,[2] summarizes the theory of fission as follows:

> Originally the star must have been single, it must have been widely diffused, and must have been endowed with a slow rotation. In this condition the strata of equal density must have been of the planetary form. As it cooled and contracted the symmetry round the axis of rotation must have become unstable, through the effects of gravitation, assisted perhaps by the increasing speed of rotation. The strata of equal density must then become somewhat pear-shaped, and afterwards like an hour-glass, with the constriction more pronounced in the internal than in the external strata. The constrictions of the successive strata then begin to rupture from the inside progressively outwards, and when at length all are ruptured we have the twin stars portrayed by Roberts and by others.

On the other hand, Chamberlin,[3] in a paper recently published by the Carnegie Institution, has expressed the view that "such rotat-

[1] *Phil. Trans.*, A, **199**, 1–53, 1902.

[2] "The Genesis of Double Stars," *Darwin and Modern Science*, p. 563 (Cambridge University Press, 1909).

[3] "The Bearing of Molecular Activity on Spontaneous Fission in Gaseous Spheroids," *Carnegie Institution, Publication 107*, p. 167.

ing gaseous spheroids must shed portions of their matter molecule by molecule, if they do so at all," and Moulton,[1] in the same volume, concludes that it is probable "that if a fluid mass ever gets into the state where fission occurs, there is at least great danger of its breaking into many pieces," and hence that "if binary stars and multiple stars of several members have developed from nebulae, the nebulae must originally have had well-defined nuclei" (corresponding to the members of the final system).

2. Between such divergent opinions, in the absence of any well-developed theory, the appeal evidently lies to the facts of observation.

Those stars which are merely double give us very little relevant evidence. The close pairs, almost in contact, revealed to us among the variable stars, may be accounted for on either theory. The apparently universal fact that the components of a binary system are comparable in mass is what might be expected as a consequence of the fission theory, but would probably have to be a postulate of the other.

When it comes to the triple and multiple systems, the situation is different. There seems to be no a-priori reason why systems originating from independent nuclei should show any definite relations of mass or relative distance. We might expect a purely random arrangement, without grouping into well-defined pairs.[2] On the contrary, such a grouping is a necessary consequence of the fission theory, depending upon elementary dynamical principles, quite apart from the more complex questions of stability, etc. It is the purpose of the present discussion to develop these consequences, and to see how far the results agree with observation.

3. In any system subject to internal forces alone the total moment of momentum about any axis through the center of gravity must remain constant, whatever the configuration assumed. There is a certain axis for which this is a maximum. Let us take this as the z-axis. Then the moments of momentum about the x- and y-axes

[1] "Notes on the Possibility of Fission of a Contracting Rotating Fluid Mass," *ibid.*, p. 157.

[2] Conditions of stability of the resulting orbits might, however, lead to such a grouping, as is shown by Moulton's work on *70 Ophiuchi* (*Astronomical Journal*, **20**, 33, 1899).

will vanish. Let us also choose the units of mass, length, and time so that the constant of gravitation is unity.

Suppose now that the system consists of two rotating bodies, of masses m_1 and m_2, in orbital motion about one another. The moment of momentum M of the system will be the sum of that due to the rotations of m_1 and m_2 about their axes, and that due to the orbital motion of their centers of gravity about that of the system. Let k_1 be the radius of gyration of the mass m_1; i_1 and Ω_1 the inclination and node of its equator on the invariable plane $z=0$, and ω_1 its angular velocity of rotation. Then its rotational moment of momentum about an axis through its center of gravity parallel to the z-axis will be $M_1=m_1k_1^2\omega_1 \cos i_1$; and similarly for m_2. We cannot express this in terms of the mean density or radius of the mass unless we know its internal constitution as well as its form; but if r_1 is the equatorial radius of m_1 (or its maximum radius, if the equator is not circular), we may set $k_1^2=c_1r_1^2$ and write

$$M_1=c_1m_1r_1^2\omega_1 \cos i_1. \tag{1}$$

For a homogeneous sphere the constant c is 0.40. Central condensation diminishes it; for example, $c=0.26$ for a sphere whose density decreases according to Laplace's law and vanishes at the surface, and 0.20 for one whose density follows the law of adiabatic equilibrium for a monatomic gas.[1]

Polar flattening has little or no influence upon the ratio c as here defined. Ellipticity of the equator again diminishes it. If the curves of equal density in the equatorial plane are changed from circles into ellipses whose minor axis is b times the major axis, c will be reduced to $\frac{1+b^2}{2}$ times its original value. This ellipticity will be greatest when the masses are homogeneous, and will increase as they approach one another.

In the extreme case of two equal homogeneous masses in contact, Darwin's calculations[2] give the maximum and minimum radii of the equator as 1.17 and 0.645, whence $b=0.55$, and $c=0.26$—an approximation only, as the actual form is only roughly ellipsoidal, but a close one. This configuration, however, is unstable. Two

[1] T. J. J. See, *Astronomische Nachrichten*, **169**, 321, 1905.

[2] *Phil. Trans.*, A, **206**, 246, 1906.

fluid masses in contact can be in stable equilibrium, if at all, only when condensed toward their centers. This condensation will diminish c, and it seems probable that for actual masses in contact c can hardly exceed 0.20, and may be much less.

The relative orbital motion may as a first approximation be regarded as in a Keplerian ellipse. Let its semi-axis major be a, its eccentricity e, and its node and inclination be Ω and i. The orbit of m_1 about the center of gravity of the system will have the semi-axis $\frac{m_2 a}{m_1+m_2}$. Its orbital moment of momentum about the z-axis will be $\frac{m_1 m_2^2}{(m_1+m_2)^2} n a^2 \sqrt{1-e^2} \cos i$, where n is the "mean motion" in the orbit. Adding the momentum m_2, and remembering that $n^2=\frac{m_1+m_2}{a^3}$, we find for the whole orbital momentum

$$N=\frac{m_1 m_2}{\sqrt{m_1+m_2}}\sqrt{a(1-e^2)}\cos i\,.$$

The expression under the radical is the semi-parameter p of the orbit.

When the masses are not spherical, these relations will no longer hold exactly. It is easy to see that both polar flattening and elongation of the masses toward one another tend to increase their mutual attraction. This will increase the mean motion, and hence the orbital momentum.

We may write

$$n^2=\frac{m_1+m_2}{a^3}(1+\zeta) \tag{2}$$

where ζ is always positive, and increases as the bodies become more elongated, and as they approach one another, varying approximately as $\frac{r^5}{a^5}$.[1] For the case already mentioned, of two equal homogeneous bodies in contact, Darwin's formula gives $\zeta=0.22$, which must be a close approximation to the truth. For two equal ellipsoids of homogeneous fluid, separated by but one-third of their longer diameters, ζ is about 0.07. If they are separated by one diameter, it is less than 0.01.

Central condensation tends strongly to diminish ζ, as the surfaces

[1] Darwin, *Phil. Trans.*, A, **206**, 195, 1906.

which inclose successive equal portions of the whole mass become smaller and more nearly spherical.

We may assume as a guess that $\zeta = 0.1$ for two gaseous masses in contact. It will be seen later that the uncertainty of its value will not seriously influence our results.

The orbital momentum about the z-axis is therefore

$$N = \frac{m_1 m_2}{\sqrt{m_1 + m_2}} \sqrt{p(1+\zeta)} \cos i. \tag{3}$$

If K is the whole moment of momentum of the system, we have then

$$c_1 m_1 r_1^2 \omega_1 \cos i_1 + c_2 m_2 r_2^2 \omega_2 \cos i_2 + \frac{m_1 m_2}{\sqrt{m_1 + m_2}} \sqrt{p(1+\zeta)} \cos i = K. \tag{4}$$

The corresponding equations for the x- and y-axes may be obtained by substituting $\sin \Omega \sin i$ and $\cos \Omega \sin i$ for $\cos i$, and zero in place of K.

4. Let us now consider an initial configuration consisting of two equal and similar masses, revolving in contact with uniform angular velocity. Let m represent their combined mass. Then

$$m_1 = m_2 = \frac{m}{2}\ ; \quad \omega_1 = \omega_2 = n = a^{-\frac{3}{2}} m^{\frac{1}{2}} (1+\zeta)^{\frac{1}{2}}\ ; \quad \text{and } r_1 = r_2 = \frac{a}{2}.$$

Also c is the same for both stars, and $e = i = i_1 = i_2 = 0$. The rotational momentum of each mass is

$$M = \tfrac{1}{8} c m^{\frac{3}{2}} a^{\frac{1}{2}} (1+\zeta)^{\frac{1}{2}},$$

and the orbital momentum

$$N = \tfrac{1}{4} m^{\frac{3}{2}} a^{\frac{1}{2}} (1+\zeta)^{\frac{1}{2}},$$

whence

$$M = \tfrac{1}{2} c N\,, \quad K = N + 2M = (1+c) N.$$

We are now in a position to follow the future course of the system. As the separated masses contract, their rotation will become more rapid, and tidal interaction will tend to transfer their rotational momentum to that of the orbital motion.

a) Let us first suppose that this process proceeds to its limit. Then all the momentum will be orbital. If p is the semi-parameter of the final orbit, we shall have $\frac{1}{4} m^{\frac{3}{2}} p^{\frac{1}{2}} = K$ (ζ vanishing in this configuration), whence

$$p = a(1+c)^2 (1+\zeta). \tag{5}$$

Setting $c = 0.2$, $\zeta = 0.1$, we have $p = 1.58a$ or $a = 0.63p$.

This assumed final condition is practically that of many binary systems. We may therefore say:

If a binary star, whose components are single, and of equal mass, has originated by fission, the distance of the centers of the two masses at the time of fission must have been fully $\frac{5}{8}$ of the present semi-parameter of the orbit (the exact ratio varying with the internal constitution of the separating masses).

If the final period is measured in years, the density at the time of separation must have been exceedingly small.[1] This was first pointed out by Moulton,[2] who discussed the case where the bodies are spheres and the orbit a circle. The equation (5) may be reduced to his equation (which involves the period), when $e=\zeta=0$, but is somewhat simpler in form.

b) Let us next assume that tidal friction is negligible. Then the moment of momentum of the detached masses remains constantly equal to M, and in time, as they contract, they may split up again. Suppose that one of them divides again into equal parts. If a^1, c^1, etc., are the constants defining the state of the new system at the time of fission, we shall have $m^1=\frac{1}{2}m$, and the whole momentum of the new system will be

$$K^1=(1+c^1)\cdot 2^{-\frac{7}{2}}m^{\frac{3}{2}}a^{1\frac{1}{2}}(1+\zeta^1)^{\frac{1}{2}}\,.$$

Equating this to its initial value M, we find

$$\frac{a^1}{a}=\frac{2c^2}{(1+c^1)^2}\,\frac{1+\zeta}{1+\zeta^1}\,. \tag{6}$$

If the law of density within the separating masses is the same as at the first fission (which seems probable if the gas is in both cases rare enough to obey the ordinary laws of gases closely), the values of c and ζ will be the same, and we shall have simply

$$a^1=\frac{2c^2}{(1+c)^2}a\,.$$

Setting $c=0.2$, we have $a^1=0.056a$.

This is a result of great importance, which may be expressed verbally as follows:

If a mass divides by fission into equal parts, and one of these

[1] Of the order of magnitude of one-millionth of that of air under ordinary conditions.

[2] *Carnegie Institution, Publication 107*, p. 133.

divides again in the same fashion, owing to its rotation alone, the initial distance of the secondary pair cannot be greater than about $\frac{1}{18}$ of that of the primary pair; and hence the mean density of the mass at the time of the second separation must be at least 2500 times as great as at the time of the first.

If tidal evolution proceeds to the limit in this new system, the final semi-parameter of the secondary orbit will be given by the equation

$$p' = a'(1+c')^2(1+\zeta') = 2c^2(1+\zeta)a\,. \tag{7}$$

If there is no tidal friction, each component will again split up into a much closer pair, until the increase of density sets a physical limit to the process.

c) If, as is most probable, tidal influence has diminished the rotational momentum of the separated masses, so that at the time of the second fission it has x times its initial value, we have only to set cx instead of c in (6) or (7) and $c(1-x)$ instead of c in (5), to find the dimensions of the corresponding bodies or orbits in terms of the initial distance a. Since in this case the final orbits need not be in the same plane, we must also set $p \cos^2 i$ in place of p in (5) and (7); and the equations of momentum about the x- and y-axes will give relations between the positions of the orbit planes.

5. The results so far obtained hold good only in the case of fission into equal parts. If the masses resulting from fission are unequal, we may set

$$m_1 = my\,, \quad m_2 = m(1-y)\,,$$

and, at the moment of separation,

$$r_1 = az\,, \quad r_2 = a(1-z)\,, \tag{8}$$

(where y and z lie between zero and unity).

Proceeding as above, keeping the subscripts for the two masses separate, and setting for brevity $m^{3/2}a^{1/2}(1+\zeta)^{1/2} = \mu$, we find:

$$M_1 = yz^2c_1\mu\,, \quad M_2 = (1-y)(1-z)^2c_2\mu$$
$$\text{and } N = y(1-y)\mu\,,$$

and hence, if tidal evolution proceeds to the limit, for the semi-parameter of the final orbit

$$p = a(1+\zeta)\left(1 + \frac{z^2c_1}{1-y} + \frac{(1-z)^2c_2}{y}\right)^2. \tag{9}$$

If tidal action does not enlarge this orbit at all, its final semi-parameter will be

$$p_0 = a(1+\zeta) . \tag{10}$$

The actual value must in all cases lie between these limits.

If the detached mass m, without loss of momentum, divides again into two portions, the ratio of whose masses is $u_1 : 1-u_1$, and tidal evolution proceeds to the limit in the resulting system, the semi-parameter p_1 of the final orbit will be given by:

$$p_1 = \frac{z^4 c_1^2}{y u_1^2 (1-u_1)^2} \cdot a(1+\zeta) . \tag{11}$$

If the rotational momentum of this mass is reduced by tidal action to x_1 times its initial amount before the second fission, we must as before replace c_1 by $c_1 x_1$ in (11), and by $c_1(1-x_1)$ in (9), and p by $p \cos^2 i$ in both. We thus obtain the equations of the most general case.

6. The radii of the two separating masses can be very approximately determined when the ratio of their masses is known.

The final severance of the neck connecting the two masses will take place at that point on the line of centers where the resultant of the attractions of the two masses is exactly balanced by the centrifugal force. The distances of this point from the centers of gravity of the masses, and of the system, are respectively az, $a(1-z)$, and $a(y+z-1)$.

Let ζ_1 and ζ_2 denote the fractions by which the attractions of the two masses at this point are increased by their departure from sphericity. (As this influence increases rapidly with diminishing distance, these will be greater than the quantity ζ, previously defined, which represents the increase of the attraction of each body on the other as a whole.)

The condition of equilibrium may then be written:

$$(1+\zeta_1)\frac{m_1}{a^2 z^2} = (1+\zeta_2)\frac{m_2}{a^2(1-z)^2} + n^2 a(y+z-1) ,$$

which reduces to:

$$\frac{(1+\zeta_1)y}{z^2} = \frac{(1+\zeta_2)(1-y)}{(1-z)^2} + (1+\zeta)(y+z-1) . \tag{12}$$

Differences between ζ_1 and ζ_2 can only arise from differences in the form and internal structure of the two masses. These will not be

great when they are comparable in magnitude. Moreover, if ζ_1 increases (other things being equal), z must increase. But increase of ζ_1 corresponds to increased ellipticity of the mass m_1, and hence to a decrease of c_1. Its moment of inertia, which is proportional to $c_1 z_1^2$, will therefore suffer little change; and this alone appears in the equations (9) and (11).

We may therefore, for our purpose, safely assume that $\zeta_1 = \zeta_2$. If now we set $\frac{1+\zeta}{1+\zeta_1} = k$, (12) becomes:

$$y\left(\frac{1}{z^2} + \frac{1}{(1-z)^2} - k\right) = \frac{1}{(1-z)^2} + k(z-1) \ . \tag{13}$$

For such mass-ratios as are known to occur among double stars, the solution of this equation is almost independent of k, as is illustrated by the following examples:

y	If $k=0$	If $k=1$	Adopted
$\frac{1}{5}$	$z=0.333$	$z=0.360$	$z=0.35$
$\frac{1}{4}$	0.366	0.390	0.38
$\frac{1}{3}$	0.414	0.429	0.42
$\frac{1}{2}$	0.500	0.500	0.50

The actual value of k will probably be much nearer unity than zero. It is clear that the values of z adopted in the last column can be very little in error.

7. With these data, we proceed to calculate the ratio of the greatest possible final distance (semi-parameter) of the close secondary pair to the least possible distance of the wide primary pair. By (10) and (11) this is

$$\frac{p_1}{p_0} = \frac{z^4 c_1^2}{y u_1^2 (1-u_1)^2} \ \frac{\cos^2 i}{\cos^2 i_1} \ . \tag{14}$$

This depends both on the ratio y of the mass in question to the original mass and on the corresponding ratio u_1 for the second fission. If the masses formed by the latter are equal, we find (neglecting the inclinations):

$y=$	$\frac{4}{5}$	$\frac{3}{4}$	$\frac{2}{3}$	$\frac{1}{2}$	$\frac{1}{3}$	$\frac{1}{4}$	$\frac{1}{5}$	
$\frac{p_1}{p_0}=$	$3.57c_1^2$	$3.15c_1^2$	$2.72c_1^2$	$2.00c_1^2$	$1.48c_1^2$	$1.33c_1^2$	$1.20c_1^2$	(15)

If the masses resulting from the second fission are unequal, these values of $\frac{p_1}{p_0}$ must be multiplied by the following factors:

Ratio of masses:	1 : 1	2 : 1	3 : 1	4 : 1	(16)
Factor:	1.00	1.26	1.78	2.44	

To find the initial distance a_1 of the close pair (at the time of fission) we see by (9) and (10) that we must divide the ratios just obtained by

$$\left(1+\frac{z^2c_1}{1-y}+\frac{(1-z)^2c_2}{y}\right)^2$$

(in which the values of y and z are those corresponding to the second fission).

Setting $c_1=c_2=0.2$ and combining the results with (16), we find that $\frac{a_1}{a}$ may be obtained by multiplying the quantities (15) by the following factors:

Ratio of masses:	1 : 1	2 : 1	3 : 1	4 : 1	(17)
Factor:	0.69	0.80	0.98	1.16	

8. The following consequences may easily be deduced from the above data:

Given a gaseous mass, which divides by fission, without external disturbance, into two parts:

1) The distance of centers at the time of separation is greater, and the density less, the more unequal these parts are.

2) The ratio in which the initial distance can be increased by tidal action increases as the masses become more unequal.

3) The smaller mass has the greater density just after separation.

4) The ratio of contraction necessary to bring about a second fission (other things being equal, and tidal friction absent) is less for the greater mass.

5) The ratio of the dimensions of the separating masses at the time of the second fission to that at the first involves the factor c^2, and is always small.

6) The same is true of the final orbits resulting from the successive fissions.

7) The increase of density between the fissions is very great.

The greatest disparity in mass of which we have satisfactory evidence among the few binary systems which have been investigated with any approach to accuracy is about 3:1. In most cases the components are much more nearly equal.

For mass ratios within this limit, the maximum ratio of the parameter of the secondary orbit to that of the primary is $5.6c^2$ which, as c is at most 0.2, cannot be greater than 0.22. The corresponding ratio of the initial distances is at most $3.19c^2$, or 0.124. As in this case the separating mass contains $\frac{3}{4}$ of the original material, its density at the time of the second fission must be at least 380 times as great as at the time of the first; which is the minimum ratio of increase.

The smaller mass produced by the original fission will give rise to a still closer pair, with greater initial density.

Tidal action before the second fission can only increase the distance of the wide pair, and decrease that of the close pairs. It is easy to see that its influence will be relatively greater upon the smaller mass. This will increase the difference in distance of the corresponding pairs, and, if powerful enough, may keep the smaller mass from dividing, and so produce a triple instead of a quadruple system.

If neither mass divides again, we find from (9) (setting $c=0.2$, $\zeta=0.1$) that the final semi-parameter of the orbit is at most equal to twice the initial distance (so long as one mass is not more than three times the other).

9. For such a distribution of masses as is found among binary stars, the results of the fission theory are quite definite. Multiple systems arising in this way must be pairs, one or both of whose components are themselves double, with a distance less than about one-fifth that of the wide pair—usually much less. Some of the components of these close pairs may be still closer pairs, after the same fashion.

It would therefore seem that we ought to be able to tell by a mere glance whether a multiple star can have originated by fission or not. But the situation is not really quite so simple.

We must first of all have some evidence (such as common proper motion) that the stars really belong together. Among the many systems of this sort there is not one in which we can yet determine the orbital elements of the wide pair; and in most cases this is still impossible even for the close pairs.

We therefore usually know, not the real distances between the stars, but only their projections on the celestial sphere, which may be much foreshortened. Moreover, on account of the eccentricity of

the orbits, the actual distances at a given time may differ considerably from the semi-parameters of the orbits.

The ratio of the observed distances of the close and wide pairs may therefore be very different from that of the parameters of their orbits. We cannot determine one from the other, in any particular case, unless we know the orbital elements; but the theory of probability enables us to estimate how many cases there will be, out of a large number, in which the apparent ratio will exceed the true one by more than any given factor.

Since we know of no reason why the smaller orbit should be more eccentric, or more highly inclined to the line of sight, than the larger, it is a priori equally likely that the larger or the smaller distance will be most affected by these influences; and hence it is equally probable that the apparent ratio will exceed or fall short of the true one; that it will be more than double or less than half the latter, etc.

In general, let r_1, r_2, denote the real, and s_1, s_2, the apparent distances, and let $f(x)$ be the probability that, owing to a given cause, $s_1 < r_1 x$. By hypothesis, the same function represents the corresponding probability for s_2. There will be certain values a and b which mark the extreme limits of the given influence (as for example 0 and 1 in the case of foreshortening). Then $f(x)=0$ if $x<a$, and $f(x)=1$ if $x>b$. The probability that $\frac{s_1}{r_1}$ lies between x_0 and x_0+dx is $f'(x_0)dx$. In order that $\frac{s_2}{s_1}$ may be less than k times $\frac{r_2}{r_1}$ we must in this case have $\frac{s_2}{r_2}<kx_0$. The probability of this is $f(kx_0)$. Combining the two probabilities and integrating over the whole range of possible values of x, we find for the whole probability $F(k)$ that $\frac{s_2}{s_1}$ is less than $k\,\frac{r_2}{r_1}$.

$$F(k)=\int_a^b f(kx)\,f'(x)dx.$$

It is easy to show that $F(1)=\frac{1}{2}$, and that $F\left(\frac{1}{k}\right)=1-F(k)$, these being merely formal statements of the propositions already proved by considerations of symmetry.

If now we have two independent influences, which separately give rise to probabilities $F(k)$ and $\Phi(k)$, of the kind just discussed, it

follows in the same way that the probability $\Psi(n)$ that under their joint action $\frac{s_2}{s_1}<n\frac{r_2}{r_1}$ is given by the equation

$$\Psi(n)=\int_\alpha^\beta F(nk)\ \Phi'(k)dk\ ,$$

the limits being those appropriate to $\Phi(k)$. As before, by symmetry

$$\Psi\left(\frac{1}{n}\right)=1-\Psi(n)\ .$$

In the case of foreshortening, if we assume that all directions of r_1 and r_2 are equally and independently probable, we find easily $f(x) = 1-\sqrt{1-x^2}$ and hence, if $k<1$,

$$F(k)=\frac{k^2}{3}+\frac{k^4}{3\cdot 5}+\frac{k^6}{5\cdot 7}+\ .\ .\ .\ .$$

In the case of an elliptical orbit, since we observe all the systems under discussion in the same relatively short interval of *time*, the probability that $\frac{r}{p}$ is less than any given value is simply the fraction of the whole period of revolution during which it is below this limit. The necessary integration is best performed graphically, and so is the combination of the result with the influence of foreshortening.

In the numerical work the orbital eccentricity has been taken as 0.5, about the average for double-star orbits.

The results are as follows:

s represents the observed distance, r the real distance, and p the semi-parameter of the orbit.

$F(n)$ is the probability that $\frac{s_2}{s_1}$ is *greater* than n times $\frac{r_2}{r_1}$

$\Phi(n)$ is the probability that $\frac{r_2}{r_1}$ is *greater* than n times $\frac{p_2}{p_1}$

$\Psi(n)$ is the probability that $\frac{s_2}{s_1}$ is *greater* than n times $\frac{p_2}{p_1}$

n	4	3	2	$\frac{3}{2}$	1	$\frac{2}{3}$	$\frac{1}{2}$	$\frac{1}{3}$	$\frac{1}{4}$
$F(n)$	0.02	0.04	0.09	0.17	0.50	0.83	0.91	0.96	0.98
$\Phi(n)$	0.00	0.00	0.10	0.20	0.50	0.80	0.90	1.00	1.00
$\Psi(n)$	0.035	0.07	0.16	0.28	0.50	0.72	0.84	0.93	0.965

10. It is now possible to compare the observed facts regarding multiple stars with the predictions of our theory.

To be sure of dealing only with physical systems, discussion has been confined to cases where there is good evidence that all three or more stars have a common proper motion. The data are those of Burnham's *General Catalogue of Double Stars*, supplemented at times by Lewis' catalogue of the Struve stars (*Memoirs R. A. S.*, **56**).

A		*B*		*C*		*D*	
s_1 LESS THAN 100 YEARS' P. M.		s_1 BETWEEN 100 AND 300 YEARS' P. M.		s_1 BETWEEN 300 AND 1000 YEARS' P. M.		s_1 GREATER THAN 1000 YEARS' P. M.	
No.	s_2/s_1	No.	s_2/s_1	No.	s_2/s_1	No.	s_2/s_1
2109*	0.07	152	0.03	1036	0.03	1448	0.13
2279	0.12	648	0.03	1457	0.08	1755	0.14
4414	0.11	672	0.06	2605	0.02	1985	0.13
4477*	0.16	1070*	0.03	2883	0.02	2027	0.005
4771*	0.07	1262	0.26	...	0.61	2406	0.04
....	0.17	1471	0.01	3099	0.01	2883	0.42
4866	0.09	1559	0.10	3402	0.33	4456	0.40
7040	0.15	2857	0.02	3962	0.04	4481	0.06
7487*	0.10	3402	0.32	4122	0.08	5833	0.06
7878	0.22	3559	0.18	5331	0.02	6482	0.02
7929	0.06	3757	0.19	5841	0.05	8783	0.02
8162*	0.04	6155	0.03	7259*	0.01	8785	0.01
8642	0.09	6296	0.04	7493	0.07	9602	0.004
9114	0.06	6571	0.05	7533	0.02	9617	0.03
10643	0.09	7332*	0.01	....	0.05	10112	0.004
11928	0.27	9011	0.04	7905	0.01	11160	0.49
13055	0.14	9090	0.10	8669	0.01	11839	0.57
13264	0.06	9643	0.04	11129	0.02	.....	0.53
		9660	0.03	11160	0.14	12573	0.01
		9782	0.10	12378	0.05		
		10057	0.15	.....	0.05		
		12257	0.01	12571	0.04		
		12946	0.13				
		13025	0.07				

The whole number of double stars for which these catalogues record common proper motion is about 800. Of these, 74 are triple or multiple, a not inconsiderable percentage of the whole.

As the full data are so easily accessible, it is only necessary to give here the numbers of the stars in Burnham's catalogue, and the ratio $\frac{s_2}{s_1}$ of the apparent distances of the close and wide pairs. These have been divided into four groups, according to the distance of the wide pair (in the case considered), measured in years' proper motion of the system—thus roughly classifying them according to their real dimensions.

As in some cases both components of a wide pair are double, and in others the wide pair itself forms one component of a still wider pair, certain systems appear two or three times in the list, raising the whole number of entries to 83.

The distance of the wide pair is measured from the mid-point of the closer pair when this correction affects the results appreciably. In a few cases, when the orbit of the close pair is known, its semi-major axis replaces the observed distance s_2. (This is chosen instead of the semi-parameter because it is nearer the mean value of r_2.) These cases are indicated by an asterisk *.

The distribution of the individual values of s_2/s_1 in each group is as follows (values at the limit between two lines being divided equally between them):

s_2/s_1	*A*	*B*	*C*	*D*	*ABC*	Theory
Over 0.40			1	5	1	1
0.40 to 0.30		1	1		2	2
0.30 to 0.20	2	1			3	3
0.20 to 0.15	2½	2½			5	4½
0.15 to 0.10	4	3	1	3	8	9½
0.10 to 0.05	8½	4	5	2	17½	16
0.05 to 0.025	1	8½	5	2	14½	8
Under 0.025		4	9	7	13	1

The average distance of the wide pair increases rapidly from group to group. This explains the deficiency in the last lines of group *A*, for close pairs of such small separation could not usually be seen double.

Apart from this there is evidence of a progressive change in the distribution from group to group. This may be partly due to the fact that foreshortening of the wide pair at the same time diminishes its apparent distance and increases the ratio $\frac{s_2}{s_1}$; which tends to heap up the larger ratios in the earlier columns of the table. The marked discrepancy for group *D* cannot, however, be explained in this way.

Setting it aside for the moment, and combining the other groups, we have in the column headed *ABC* the observed distribution for all systems whose extent is less than 1000 years' proper motion.

The last column gives the distribution which might be expected,

according to paragraph 9, among 45 systems for all of which $\frac{p_2}{p_1}=0.09$. The agreement is very good, except in the last two lines. That is, we may account for the observed facts by assuming that in 45 of the systems $\frac{p_2}{p_1}=0.09$, while in the other 19 it is very much less, averaging about 0.02.

Of course it is not to be supposed that $\frac{p_2}{p_1}$ has this exact value in all these cases. The point is that in order to account for the facts we are not obliged to assume that it is ever greater than 0.09, while we are forced to conclude that it is often very much less. This is exactly what the fission theory demands, and so far it accounts completely for the facts.

For the stars of group *D* the proportion of large values of $\frac{s_2}{s_1}$ is far too great to be explained in this way. In fact, they fall sharply into two groups, with this ratio less than 0.15 or greater than 0.40.[1] The former can easily be accounted for on the fission theory, but the latter cannot. It would seem that at about this limit of distance we begin to come upon systems of different origin—perhaps evolved from separate nuclei in the original nebula, as suggested by Moulton.

These wide pairs, however, with a separation of many thousand astronomical units, showing as yet no sign of relative motion, whose periods, if they are really in orbital motion, must be counted by hundreds of thousands of years, are very far from what are usually called binary stars. It is probable that if we could extend our survey to systems of still greater linear extent we would find them grading into the irregular star-clusters, like the *Pleiades*, whose members have a common proper motion.

The average separation of the 50 pairs (close and wide) which show sensible relative motion is but 20 years' P. M., and the greatest among them 71 years' P. M. Of the 16 pairs which are moving at the rate of 1° per year or more, the greatest separation is 27 years', and the average only 6 years' P. M. These systems therefore lie far within

[1] The familiar "Trapezium," θ *Orionis*, which was not included in the above discussion on account of the smallness and uncertainty of its P. M., would fall in this latter group, and increase the force of the argument.

the limits of distance up to which the fission theory accounts for the phenomena.

11. When both the wide and close pairs are in motion, a further test of the theory is possible, for it demands that both pairs shall revolve in the same direction—though not necessarily in the same plane. In seven of the eight cases in our list the apparent motions are in the same direction. In one, ξ *Scorpii*, they are different; but a single case of this sort is not surprising, for occasionally the planes of the two orbits will pass on opposite sides of the sun, so that, as projected on the sky, they are apparently described in opposite directions.

The numerous cases in which one or both components of a visual binary are spectroscopic binaries (usually of short period) are also in agreement with the fission theory—representing cases where tidal friction has considerably retarded the rotation of the separated mass before its second division.

The few examples of stars spectroscopically triple (*Polaris*, *Algol*) show a short period superposed on a very much longer one, and we meet again the close and wide pairs of the theory.

In the entire range of triple systems which are accessible to observation, we therefore find everything in harmony with the fission theory, up to a distance exceeding more than tenfold that of the widest pairs which so far show signs of relative motion.

It may still be questioned whether the examples which have been discussed are sufficiently numerous and typical to be representative of binary stars as a whole. In answer to this it may be said that there are 34 binary stars whose orbits, according to Burnham, may be regarded as fairly well determined. Eight of these are visual triples, which appear in the preceding table, and the bright components of two more are spectroscopic binaries, so that 30 per cent. of the whole give direct evidence favorable to the fission hypothesis. It is probable, in view of the limitations of our observing powers, that the actual percentage of triple systems is considerably greater. They are certainly enough anyhow to give a good sample of the whole. The singleness of the components of the remaining binaries in no way discredits the theory, but, according to it, is a result of relatively great tidal action.

12. As the fission theory accounts so well for the observed facts, it is worth while to consider the objections to it in some detail.

a) As regards the stability of such dividing masses as are here postulated, practically nothing is known theoretically, owing to the great difficulty of investigation. The facts already detailed establish a presumption that when the subject is mathematically explored, some series of figures of equilibrium of a *compressible* gas, ending in fission into two comparable masses, will be found to be stable. The continued existence of many variable stars, such as *β Lyrae*, whose light-changes can be best accounted for on the hypothesis that they are composed of two ellipsoidal masses of very small density revolving practically in contact, raises this presumption to a very high degree of probability; and it may well be accepted, on these physical grounds, unless direct mathematical evidence is produced to the contrary.

b) Professor Chamberlin's theory of the escape of gas, molecule by molecule, from the equatorial region of a rotating spheroid presupposes that the velocity of the gaseous molecules in question, relatively to the neighboring gas as a whole, is greater than the difference between the rotational velocity of the surface and the orbital velocity of a particle revolving in a circle under gravitation just outside. His conclusion is that "the critical stage of exact balance between the centrifugal and centripetal factors of the spheroid is never reached. If so, bodily separation is excluded by the conditions of the case."[1]

Such loss of gas may of course occur; but its amount, which will depend upon the temperature and surface-gravity of the mass, can hardly be predicted. It would have to be very considerable to prevent the arrival of the mass at the critical stages preceding fission, which consist in deep-seated changes in the distribution of its matter, which come into play long before the centrifugal force at any point on the surface equals the attraction, and while the difference between the rotational velocity of the surface and the orbital velocity of a particle just outside is a considerable fraction of the latter.

The lightest gases, hydrogen and helium, would in any case be lost first. But in stars such as *β Lyrae*, which are apparently almost in the act of fission, these are just the gases which are spectroscopically most prominent. In general, the extreme rarity of stellar spectra

[1] "The Tidal Problem," *Carnegie Institution, Publication 107*, p. 167.

which lack the lines of hydrogen seems conclusive evidence that this process is of little importance in stellar evolution. A star whose hydrogen had escaped into a sort of ring of loose molecules surrounding its equator would show the hydrogen lines if viewed from a point near its equatorial plane, but not from the direction of its pole; and numerous cases of this sort ought to occur if the phenomenon was at all common among the stars.

c) Professor Moulton's argument against the fission theory may best be given in his own words.[1]

At the time of fission all parts are rotating at the same angular rate, and one of the two parts must have a mean density less than, or at most equal to, the mean density of the original mass. Consequently one of the two fragments, because of its lower density and equal rotation, must have at least as great a tendency to fission as that which led to the division of the original mass, unless either its form is one of greater stability, or the tidal forces of the other member of the pair tend to keep it from breaking up. If, as seems probable, the approximate spheroid is the most stable figure of equilibrium, and if the mass under consideration has suffered fission by evolution along this line of figures, as is assumed, then the former alternative is eliminated. It does not seem that the tidal factor can tend towards stability. . . . . We observe next that the binary stars are now actual stars of considerable density. Consequently if they have originated from the fission of nebulas they have undergone enormous contraction. The contraction implies increased rotation which would increase the already dangerous tendency for at least one part to suffer further fission. Tidal friction would offset this tendency by decreasing the rotations, but considering all the factors involved, it is seen that if a fluid mass ever gets into the state where fission occurs, there is at least great danger of its breaking into many pieces.

Before discussing this, it is well to consider for a moment the consequences of loss of stability in such a case. Whenever "exchange of stability" occurs, the result is a *change of form* (such as that from a spheroid of revolution to an ellipsoid, in the case of a homogeneous fluid). The new form at the start differs but infinitesimally from the old, but deviates from it more and more as contraction proceeds. At first it increases in stability, but later it may become unstable, and go over into still another form, and so on. If contraction ceased at any time, the mass would remain permanently of the form which it then possessed.

All through this sequence the succession of figures of equilibrium

[1] "The Tidal Problem," *Carnegie Institution, Publication 107*, pp. 156–157.

is *physically* continuous, though the mathematical expression for them changes abruptly when exchange of stability occurs. It is of course possible that the series might terminate at some point beyond which no stable figures of equilibrium existed; but, as we have seen, the stars themselves furnish evidence against this.

So long as this does not happen, the question of physical importance is not: Is the form approaching instability? i. e., Is a change in the mathematical expression for it imminent? but: How far has it gone in the orderly series of evolution forms which begins with the spheroid and ends with two separate masses?

The increase in the density of the gas while passing through this series of forms may be, and in all probability is, very great, and the time consumed in the process indefinitely long.

The determining factors in the physical problem, where the mass is given, are not density and angular velocity, but density and moment of momentum. Bodies of the same mass, but with different moments of momentum, will reach similar forms at densities which vary inversely as the sixth power of the latter.[1]

Now there are often two figures of equilibrium, of different forms, which have the same mass, density, and angular velocity, but differ considerably in moment of momentum. On this account they cannot represent different stages in the history of the same system; but it is evident that the one with the smaller angular momentum represents a system in an earlier stage of evolution than the other.

For example, in the case of a homogeneous fluid, the ellipsoid of three unequal axes, when just ready to change into the pear-shaped figure, is so much flattened that its longest axis is 2.90 times the shortest one, about which it rotates. But there exists also a spheroid of revolution, highly stable, with the same density and angular velocity, whose equatorial radius is 1.42 times the polar. If the two are of equal mass, the moment of momentum of the first is 1.67 times that of the second. It follows that the spheroid will not assume the form of the ellipsoid until it has contracted to 22 times its original density.

[1] Let M be the moment of momentum, m the mass, and r any linear dimension. Then, for similar forms, $M \propto mr^2\omega$, $\rho \propto \frac{m}{r^3}$, and $\omega^2 \propto \rho$. Hence $M \propto m^{\frac{5}{3}}\rho^{-\frac{1}{6}}$, and $\rho \propto \frac{m^{10}}{M^6}$. (Cf. Moulton, *op. cit.*, p. 148.)

The case of the masses produced by fission is exactly similar. Just before separation the shape of the whole mass is much like an hour-glass. Just afterward the shape of the detached pieces is much like an egg. The density and angular velocity are practically unchanged. Consider *one* of these eggs, and imagine an hour-glass of the same mass and density (at corresponding points). The angular velocities of the two will be the same, but it is obvious, almost intuitively, that the moment of momentum of the hour-glass must be much the greater of the two. The egg then, considered by itself (without thought of its origin or of its twin egg), represents a much earlier stage of evolution than the hour-glass, and will have to contract a great deal before it reaches a similar form and divides into two. The detailed calculations of momentum in paragraph 7 show that its density must increase several hundred fold before this happens.

It therefore appears that the fear that a mass liable to fission would soon break up into many pieces is without foundation.

The final density might in some cases be so great that the gas laws no longer held in their simple form, and the character of the figures of equilibrium was changed. But in the case of all double stars, except spectroscopic binaries of short period, the mean density of the mass at the time of separation must have been much less than that of air under ordinary conditions, so that the effects of departures from the gas laws would probably be insensible.

It may be appropriate to add that the argument here discussed has no bearing upon the main thesis of Professor Moulton's paper in which it appears, and that the equations of paragraph 5, if applied to the earth and moon, may be used to illustrate the difficulties which attend the hypothesis of their origin by fission, in very much the way in which he has discussed the problem.

13. The alternative theory—that multiple stars have developed from nebulae which originally had well-defined nuclei, corresponding to the members of the system[1]—must in any case be invoked to account for those wide and irregular groups (such as the Trapezium in *Orion*) for which the fission theory gives no explanation. But the arrangement in close and wide pairs, so characteristic of the large majority of multiple stars, is on this theory a positive difficulty. Not only is

[1] Moulton, *op. cit.*, p. 157.

there no apparent reason for it, but if we try to retrace in imagination the history of such a system, through stages of greater and greater diffusion as we penetrate farther into the past (keeping in mind that the moment of momentum of the whole system must remain constant), it is hard to form any idea of the history of the nuclei which finally form a close and rapidly revolving pair, attended by a distant companion.

In any case, by this theory (in Professor Moulton's words) "we do not explain anything—we only push by an assumption the problem of explaining the binary systems a little farther back into the unknown."

The fission theory, on the other hand, not only accounts for the existing peculiarities of arrangement, but gives a simple and fairly detailed account of the manner of their origin.

While, therefore, it is apparently necessary to assume that star-clusters have developed from originally separate nuclei, it is more reasonable to suppose that binary stars have originated by fission: and this theory may well be adopted as a working hypothesis until some evidence, either observational or theoretical, is produced to oppose it.

PRINCETON UNIVERSITY OBSERVATORY
January 26, 1910

OBSERVATIONS OF THE AURORA, MADE AT THE YERKES OBSERVATORY, 1902–1909

By E. E. BARNARD

In the *Astrophysical Journal*, **16**, 135–144, October 1902, I have given my observations of the aurora at the Yerkes Observatory in the years 1897–1902. I have continued these observations, but not with the closeness of the previous records. I have not failed, however, to record every aurora that I have seen.

During this time there have been few striking auroras, with the exception of that of October 30, 1903, which was comparable to some of the brighter ones seen in the early years of the Yerkes Observatory. From newspaper accounts, this aurora was widely extended over this country from New York to Oregon. This was one of the large magnetic disturbances that once in a long while cripple the telegraph systems of the country. A few of the newspaper accounts of this disturbance are worth preserving and I have copied some of them here.

From the Chicago *Daily News*, Saturday, October 31, 1903, 5 P.M. edition:

> Peculiar electrical effects were experienced in Chicago today. Frisky waves of electric force zigzagged through the atmosphere and at times caused temporary suspension in the transmission of messages over telegraph wires. . . . .
>
> TELEGRAPH WIRES MADE USELESS
>
> Beginning shortly after midnight the waves scattered all over the country. The Western Union Telegraph Company's western wires, extending as far as Denver, at intervals were rendered useless. Messages in the process of transmission were stopped. Postal Telegraph wires were affected but not disabled. The French Cable Company reported disturbance in its service. . . . .
>
> By noon the effect in Chicago had subsided and all telegraph wires were nearly normal. Ten years ago was the last time any similar disturbance was experienced here, according to L. K. Whitcomb, chief operator of the Western Union Telegraph Company. . . . .
>
> The French Cable Company, according to an Associated Press dispatch from New York, gave notice early that owing to extraordinary electrical disturbances it was informed by the European administration that business will be subject to heavy delay.

AURORA BOREALIS IN NEW YORK

New York had an aurora borealis display early today, says another Associated Press dispatch. It interfered with telegraphic service. Both telegraphic companies reported wire trouble on account of the electrical display, and all cablegrams were accepted subject to heavy delay. This was the first [?] display of the aurora in New York for ten years and it lasted several hours.

NORTHERN LIGHTS IN MINNESOTA

From Duluth, Minn., comes a report through the Associated Press that the northern lights illuminated the heavens for half an hour about midnight. The display was pronounced by several to be the most beautiful ever seen in Duluth. It took the form of huge, waving plumes, blown by the wind, the tips extending directly overhead.

Another paper, elsewhere, reports the following:

Salt Lake (Utah), October 31.—A remarkable display of the aurora borealis was visible here early this morning, continuing for several hours. So intense was the light that many were of the belief that a large fire was raging north of the city. Telegraph service throughout the Northwest was badly crippled for some time.

Seattle (Wash.), October 31.—The display of aurora borealis probably reached its climax as viewed in the Puget Sound country. The display lasted over two hours. The city was illuminated as if by moonlight. The rays met in a focus in the zenith. The coloring was mostly brilliant emerald and blood red. The effect was startling.

Portland (Ore.), October 31.—The aurora borealis was observed here during the early part of last evening for the first time in about ten years. A dense fog settled down over the city soon after dark and brought the display to an early termination. The peculiar electrical conditions greatly hampered the telegraph companies in the transmission of messages.

A striking feature of this aurora, as seen at the Yerkes Observatory, was the rapidly ascending waves of light that succeeded each other with a startling rapidity. They had their origin apparently in the upper part of the bright arch, as if thin rims of the arch were expelled upward. This spasm occurred at intervals, and would last for a minute or so, and then cease for a short time, as if to renew its energies for another display. These waves shot up to beyond the zenith with very great velocity, succeeding each other at intervals of about a third or a fourth of a second.

Some of the phenomena of the previous auroras have not been repeated; perhaps from the fact that in general these later auroras

have been of a milder type. And yet this period has included a sun-spot maximum. In these last observations I have not again seen the bluish-green masses which moved along the summit of the arch toward the east and which were such striking features of the aurora of February 11, 1899.

One fact that is evident in the observations made here is that the auroral arch varies greatly in size. For if we assume that it is the segment of a circle whose center is essentially stationary with respect to the earth it must fluctuate as much as a diameter of itself in size. The arch sometimes extends half-way to the celestial pole, and at other times its entire extent is beneath the horizon, as indicated by the streamers coming up from below the horizon.

Perhaps with the exception of the narrow ray—a degree in width and eighty degrees or more in length—which was seen rising from the west horizon on August 21, 1903, no new phenomena have been seen.

The streamers which spring from the arch as a base, and which always have a decided lateral motion and last for a minute or so only, almost always move to the west. On several occasions, however, I have seen them divide the arch, with respect to their motion, so that the ones to the west moved west and those to the east moved east. This is very rare. The motion is about 2° in one minute (and not 2 minutes to the degree as I stated in *Astrophysical Journal*, **16**, 143). I have wished to determine this motion more accurately, but we have had so few ray-producing auroras in late years, and the rays are so transient, that I have not been able to do so. It would be interesting to know if this motion is constant in a streamer and for all streamers.

The pulsating bright masses that usually appear in the northeast or northwest, but which are sometimes seen under the pole, are among the most interesting phenomena. They are sometimes present when there are no other evidences of an aurora.

In the auroras seen here there is comparatively little in the way of color. Referring again to the great aurora of April 16, 1882, which I saw at Nashville, Tenn., there were magnificent brilliant crimson curtains in the west, and brilliant colors in other parts of the sky. But as seen from the Yerkes Observatory in the past ten

or twelve years, there have not appeared any such striking colors. [This was written before the great aurora of October 18, 1909.]

In some of the auroras seen here, there was a double arch—one several degrees higher than the other.

Extracts from my notes now follow, in continuation of those in the paper already referred to. Time of the ninetieth meridian ($6^h\ 0^m$ slow of Greenwich mean time) is used throughout.

1902

November 23. $6^h\ 30^m$: Faint auroral light in northern horizon. $7^h\ 20^m$: Strong auroral arch, very low. Top of brightness half-way to β *Ursae Majoris*. Dark under arch. No decided streamers, but bases of streamers moving to left. $8^h\ 0^m$: The aurora had almost died out. There was still a feeble arch. $13^h\ 0^m$: There was still a feeble glow.

November 24. $11^h\ 40^m$: At this time there were auroral streamers shooting above the northern horizon. The arch seemed to be below the horizon. For several hours there had been a feeble glow but no streamers or arch. $11^h\ 30^m$: The aurora seemed to have all died out.

1903

August 21. $9^h\ 0^m$: A rather strong auroral glow in the north. $10^h\ 30^m$: The aurora was active. There was a poorly defined arch very low; estimated 5° high, with frequent streamers moving to the left. Specially active under the handle of the Great Dipper. It was so bright that it strongly illuminated a few clouds overhead. $10^h\ 50^m$: There was a strongly defined streamer near *Arcturus* in the west. $11^h\ 35^m$: The aurora had died down. For the past hour it had been quite active, and for the past half-hour there had been rapid fluctuations ascending vertically. This activity gradually passed to the right. The sharply defined streamer in the west remained stationary for a quarter of an hour. A second one a little to the south of *Arcturus* sprang up and was inclined 20° from the vertical toward the south. It was faint at first, then became brighter—very thin, and rose to the altitude of *Vega* and a little to the left of that star. It was most remarkable, not being over 1° in width. It stretched up for 80° or so, and gradually became curved—convex to the south or left and then broadened out and finally faded from view. $11^h\ 45^m$: The aurora had quieted down. There was considerable light in the northeast and a great amount of fluctuating light, especially in the northeast. The streamers to the left of the pole moved to the left. The arch was not definite. The display extended over a long reach of the horizon. The greatest activity seemed to be near 11^h. The light was much broken by luminous regions. Mr. Sullivan says it started at dark. $12^h\ 30^m$: There was a strong auroral light all over the north but it was not active. It was active again with streamers at about 14^h. This was the first aurora I had seen for a large part of a year.

September 18. $14^h 45^m$: There was a low aurora with an ill-defined arch—dark part 4° high, and a brighter rim 2° wide above this. It got a little brighter but by 16^h was very much faded out. No activity.

September 19. $7^h 0^m$: There was an ill-defined arch 6° high with another arch of light above, but not bright. By $8^h 0^m$ it had brightened considerably. $9^h 0^m$: The dark arch was 6° high with its center 10°–15° to east of the north point. It was fairly well defined. The light was very much stronger but did not extend far above the dark arch. At $9^h 30^m$ there were a few feeble streamers, after which the arch broke up and disappeared. By $10^h 0^m$ it had all gone except a feeble suggestion of a diffused glow in the north—no arch.

September 23. At a little before 16^h there was a very feeble auroral glow low in the north.

September 28. A slight aurora about midnight.

October 30. At $15^h 0^m$ a magnificent aurora was in progress. Mr. Sullivan says that there had been an arch, not conspicuous, from early in the night and that it had culminated in a great display at $15^h 0^m$. I first saw it at $15^h 30^m$. The whole north was full of great streamers and rapid pulsations were fluttering all over the sky. The greater part of the heavens—especially north—was full of hazy light, and masses of streaky brilliancy were conspicuous at different points. These streaks ran toward the north. The streamers seemed to converge toward the zenith where frequently masses of curved and irregular light appeared, fluctuating brightly and then drifting to the south. The fluctuations extended down to and below *Orion*. The streamers were broad—a few brilliant ones were narrow. They did not make much inclination to the horizon except to the northeast and northwest, and at their upper parts they seemed to diffuse toward the zenith, as if struck by a wind. For a certain height the streamers were straight, then they seemed to lose their force and become subject to drifting and irregularity. The ascending pulsations came from the north—not in arch form exactly. They would go to the zenith and beyond with extreme rapidity. There were several waves a second, but the irregular pulsations would last only $\frac{1}{10}$ second. There was frequently a good deal of red and a tendency of the streamers to form curtains. But there was no definite arch—it was diffused and extended, 10° or 15° high. The sky, except overhead and north, was more or less covered with smeary clouds, especially from *Orion* south. Though these were illuminated when the light was strongest they did not show the effect of the pulsations which seemed to pass by them in waves, showing that the pulsations were beyond them. There were a few streaky clouds in the north below the pole. I could not be positive that there was any of the auroral light this side of them. Several bright streamers were occulted by these clouds and everything seemed to be beyond the clouds. The activity extended nearly from east to west, through the north. At any one time the activity would be confined to either the northwest or northeast.

The streamers to the west of the pole moved west about 2° a minute; those to the east moved to the east. I watched this carefully to see where the line of demarkation occurred. Usually here the summit of the arch is from 15° to 20°

east of north, and one would suppose that there would be a stationary point for these streamers at the summit of the arch. Though there was no definite arch to go by I noticed that some of the streamers west of where the summit of the arch ought to be moved east as shown by the notes.

$15^h 30^m$: The whole north luminous with streaky streamers; patches of streaky light overhead and north. Some of these appeared very much like the streakiness of the *Merope* nebula in photographs of the *Pleiades*. Rapid pulsations everywhere. $15^h 50^m$: Very active. Red in north. There was no distinct arch. $16^h 5^m$: Red in northeast. $16^h 10^m$: Dying down. Most active in northwest. $16^h 25^m$: Little activity at this time—quieting down. $16^h 35^m$: Streaky streamers in the north. Not much activity; no fluctuations. $16^h 40^m$: A fine streamer appeared $5°$ west of *β Ursae Minoris*, and moved toward *β* and to the east. $16^h 43^m$: A streamer shot up through *Polaris* and moved to the east. Aurora active, with pulsations. Fine streaky streamers all over the northeast. $16^h 50^m$: Active, with big broad streamers all east of north—none to the west. No pulsations, but active pulsations a few minutes later. $16^h 55^m$: Pretty nearly died out. $17^h 10^m$: All out at this time except some luminous appearance in the north.

The pulsation periods, or periods of activity, were spasmodic—that is, every once in a while the ascending pulsations would be at work and then quit. This was the finest display I have seen since September 9, 1898. At the brightest the light did not cast a shadow.

October 31. $8^h 30^m$: There was an arch visible in the north reaching one-third of the way to *Polaris*, not very definite on account of moonlight. It was dusky beneath and the summit was $15° \pm$ east of north. Some streamers shot up at different points on the arch, but a hazy sky and nearly full moon prevented any display to amount to anything. By $9^h 0^m$ the activity had ceased, but the arch remained feebly. This soon disappeared and did not return during the night. $11^h 15^m$: No trace of aurora. $14^h 30^m$: Thick sky with few stars. No aurora. $17^h 0^m$: No trace of aurora. Sky misty but more or less clear.

November 1. No aurora.

November 17. $9^h 0^m$: A slight aurora low in the north. There were a few streamers, but there did not seem to be any arch and the streamers appeared to come from the horizon. The aurora seemed to die away soon. An hour later there was none. Frequent looks after midnight did not show any further aurora in the north. My view was confined to the immediate north. $17^h 30^m$: There was no aurora in the north but there was a large curved half-lune of pulsating light resting on the north by east horizon and curving up through *Cassiopeia* to $5°$ or $10°$ under the pole. This slowly fluctuated in brightness and was tinged with warm color. Temperature $+10°$ F.

November 18. $6^h 0^m$: A slight aurora in the north. This rapidly brightened and formed an arch, not very dark beneath. A half-hour later there were feeble efforts at streamers. There were two clouds of light at about $25°$ altitude, one in the northwest, the other in the northeast. They would brighten up, then fade

away. The one in the northwest was the most persistent. These faded away soon but a fragment of an arch formed half-way from the true arch to the pole, under *Polaris*, then it died away. At $7^h\ 0^m$ there was an auroral glow but not active in the north. Temperature $+14°$. $7^h\ 30^m$: There was a rather dull aurora with low arch, but no streamers. $12^h\ 0^m$: There was only a glow in the north. Nothing in the east or west. $17^h\ 0^m$: There seemed to be no aurora.

November 19. Mr. Sullivan reported that there was a strong arch at 8^h to 9^h, brightest at 9^h, after which it disappeared. There were no streamers.

December 29. $15^h\ 30^m$: A strong aurora in the north. It was badly broken by clouds, so one could not make much out of the arch. Feeble effort at streamers.

1904

April 18. $8^h\ 40^m$: A strong auroral arch in the north. The aurora was strongest about midnight. It seemed to disappear about $15^h\ 30^m$ just before dawn. It did not become active, except that a few feeble streamers were attempted after midnight. The arch was a low one—not among the lowest, however. The summit was some 20° to 30° to the right of the pole.

April 19 (R). Aurora from 7 P.M.

June 15. $9^h\ 0^m$: A strong auroral glow in the north. By 10^h and 11^h it was very strong with very low arch. There was some activity—efforts at streamers. At $14^h\ 0^m$ there were rapidly appearing and disappearing long luminous spots to the west of the pole at 10° or 15° altitude.

June 16. In the first part of the night there was no aurora, but there was possibly a slight auroral glow about 13^h.

October 5. $14^h\ 0^m$: A faint auroral light low on the northern and northeast horizon.. It had been visible for half an hour or more. No arch. Not anything seen of it in early part of night.

October 6. $8^h\ 30^m$. A feeble auroral glow at northern horizon. $9^h\ 0^m$: The arch was forming. $9^h\ 27^m$: Arch active. Short and rapidly changing streamers. $10^h\ 10^m$: The arch was very low. It was not strong and there was almost no activity. $10^h\ 25^m$: The arch had risen decidedly, was better defined, and was brightening rapidly. There was a second arch above it, but not so well defined. $10^h\ 30^m$: The upper arch was very bright also but not active. $10^h\ 35^m$: The second arch had almost disappeared; the main one was fainter.

When clouded at $13^h\ 30^m$ the arch was very bright but not active. The aurora seemed most active from about $9^h\ 30^m$ to $10^h\ 0^m$. There was quite an effort at streamers, some of them were distinct and long, and those under the pole moved to the left.

October 11. $11^h\ 0^m$: A slight auroral glow a little to the east of the northern horizon. $13^h\ 0^m$: It had almost entirely died away.

November 3. $14^h\ 30^m$: An auroral glow, not seen at any time earlier tonight. This then formed into an irregular brightening along the northern horizon, but no distinct arch. When I quit after moonrise, at $15^h\ 30^m$, it was quite brightish, but without activity.

November 5. $11^h\ 0^m$: A decided auroral glow at the northern horizon, extending 15° or 20° high. Unmistakable. No arch.

November 15. After moonset, at midnight, there was a strong aurora. No definite arch but dark underneath and very low. $13^h\ 0^m$: The arch was very strongly marked and dark beneath. It was very low. The bright part was not broad and was quite bright. No action. $13^h\ 25^m$: The arch was much broken up into bright masses. $13^h\ 50^m$: There was a thin streaky second arch above but no streamers. $14^h\ 35^m$: Some effort at streamers. $14^h\ 50^m$: It seemed to have died out, and dusky, hazy patches were over the brightest part which was all broken and smeary. $16^h\ 42^m$: It still feebly existed. $17^h\ 10^m$: It had disappeared.

1905

February 1 (R). Aurora from $9^h\ 30^m$ to $10^h\ 30^m$.

February 2 (R). Bright aurora all night.

March 1 (R). Auroral arch from $12^h\ 30^m$ to 15^h. No streamers were seen.

March 3 (R). Faint aurora from $10^h\ 30^m$ to 12^h.

September 25 (R). Aurora nearly all night after 10 P.M.

November 26. $8^h\ 35^m$: There seemed to be a feeble aurora near the northern horizon.

The observer was absent at Mount Wilson, in California, from January 1 until the middle of October. The observations marked (R) are from the meteorological records of the Yerkes Observatory.

A few other records so marked (R) were either missed by the observer or made in his absence.

1906

February 18. Heavy aurora at midnight. Did not notice any streamers. Very bright glow to nearly an arch. Did not look out earlier, and did not look again until $16^h\ 0^m$; then it was heavily clouded.

Mr. Stillhamer says there were two peculiar straight vertical rays, one east of north, and the other west of north. They were not visible at the same time.

February 25. $8^h\ 0^m$: Strong but low auroral glow along northern horizon. $13^h\ 0^m$: It was very much stronger and much broken as if it had been active. Some activity with attempt at streamers. $14^h\ 0^m$: About the same. $15^h\ 0^m$: The aurora had died down to a distinct, but not strong arch about 4° or 5° high.

March 24. $9^h\ 0^m$: A strong aurora with one or two streamers. By midnight it had died down to a faint glow and by 15^h it had disappeared.

May 18. $8^h\ 45^m$: A strong aurora started. No definite arch but strong light with ill-defined, uncertain dark arch beneath. $12^h\ 0^m$: It was very strong and a strong arch had formed. An hour before it was almost dead. There were no streamers. Mr. Sullivan saw several streamers in first part of the night. The aurora remained strong until daylight, but no streamers.

May 19 (R). Aurora 9^h to 13^h.

June 1. 13^h: A strong aurora, without any well-defined arch. There were some streamers.

August 12. $9^h\ 40^m$: There was an active aurora; broad sheets of light shooting up. A few minutes later it had died out except for an auroral glow which remained unchanged as late as $11^h\ 0^m$. An hour before $9^h\ 40^m$ there were no signs of an aurora.

September 22. $12^h\ 0^m$: A faint aurora in the north. Mr. Sullivan says that in the first part of the night it formed an arch but no streamers.

September 23 (R). Aurora.

September 24. $10^h\ 30^m$: There were fluctuating spots of light above the northern horizon. Later a strong light above a dark region. This died out and lighted up again farther to the east, where it was very strong in the northeast but not under the pole. It was a very strong light all along above a bank of what appeared to be dark clouds, but a star was seen through it. $13^h\ 30^m$: A feeble scattered glow. At $14^h\ 0^m$ the aurora was showing spasms of renewal—bright spots appearing and disappearing rapidly under the pole and to the east. The aurora was very fitful.

October 13. $7^h\ 5^m$: A very feeble auroral glow east of the north point. $16^h\ 50^m$: It died out soon, no increase of its light—simply a feeble glow.

1907

February 7. 8^h or 9^h: There was a strong aurora—very dark below. I did not see any streamers. Could not watch it because I was photographing. At midnight it seemed to have died out.

February 9. Sky cleared suddenly just before 7^h. The whole northern heavens from the east point to the west and to the zenith was covered with a very bright aurora—just a uniform light. In a few minutes this became very active. A very large arch half-way to the pole formed, then broke up into cloud masses which extended all over the north. Bright masses and great streamers all over the north. Then a very low arch formed, quite definite, dark below. Then this faded out and a somewhat larger arch formed, dusky beneath. Then later this disappeared and there was only a uniform glow over the northern horizon. At midnight it was very dim and quiet. Cloudy from north with haze all over sky. This aurora was the most active in several years but it did not compare with the magnificent one of October 30, 1903.

February 10. 9^h: A spot of auroral light 6° to 8° in diameter in the northwest, some 15° high. This would become very bright and then in a few seconds disappear entirely. $15^h\ 0^m$: A bright mass of fluctuating light 6° to 8° in diameter was visible in the low west. *Jupiter* was exactly half-way to the horizon from it and directly under it. The light was at 6° or 8° altitude. It became intensely bright once in a while, of a bluish-green color. Under the pole near the horizon there was frequently a long spot of light (not bright) that came and went.

February 11. An aurora was observed here through the clouds at 10^h, 13^h, and 17^h.

February 12. $14^h\,30^m$: A pretty large spot of light in the west by north, very near the horizon, that flared up very bright every few seconds, then faded out again, and a faint auroral glow near the northern horizon which when brightest showed a low arch, dark beneath. $16^h\,15^m$: A feeble, very low, long spot due north that pulsated every few seconds. I did not again see the spot near the northwest horizon.

February 13. Professor Frost reported that at different times in the night he saw spots of auroral light shining through the clouds—not through breaks.

February 14. At $7^h\,15^m$ in moonlight (moon very thin) there were several streamers in the north and northwest. These seemed to come from the horizon, there being no arch. I saw no more streamers, but later there was a decided auroral glow in the north. This was unmistakable as late as $13^h\,40^m$.

February 16. From $10^h\,55^m$ to $12^h\,20^m$ an auroral glow was visible in the north.

February 17. A feeble auroral glow in the north during the night.

February 19. $8^h\,30^m$: There seemed to be a faint aurora before it got too cloudy. Clouded at $10^h\,37^m$ and remained so.

February 20. $13^h\,30^m$: Before moonset there was a low glow near the northern horizon. At $15^h\,0^m$ there was a brilliant cloud close to the eastern horizon. Sighting along the Bruce Observatory (running east and west) it was seen that this cloud was perhaps $1°$ north of the east point—almost exactly at the east point and only $2°$ or so high. It was $5°$ or so in diameter. This died out a few minutes later and did not reappear, but there were several thin bright strips just above the northern horizon that came and went. At $16^h\,20^m$ there was a long, brilliant cloud in the northwest, low and slanting up at its north end. Its south end was near the horizon under *Castor* and *Pollux*, while its north end was above *Capella*. The aurora was then dead under the pole. This cloud died out completely by $16^h\,35^m$. At $17^h\,0^m$ there was no trace of aurora anywhere. $17^h\,30^m$: Still no traces of the aurora.

February 21. Between 15^h and 16^h there was a display of long, bright masses under the pole and near the horizon. These would brighten up and then fade out. They appeared like horizontal bright clouds. I did not see anything of bright clouds in the east or west but I only glanced out once in a while, so they may have been there in the intervals. At $16^h\,45^m$ it seemed dead under the pole.

March 5. It was reported that there was an aurora last night—that is, evidences of aurora.

March 10. A strong aurora at $7^h\,30^m$. At $8^h\,10^m$ it was sending up a number of streamers. It was more or less active till $9^h\,30^m$, when it seemed to have died out. There was no distinct arch, unless it was hidden by some low clouds. At $10^h\,30^m$ there was perhaps a feeble glow, but the aurora was dead. The arch was again strong at about 12^h. At 15^h–16^h the aurora was again evident as a glow.

March 19. $13^h\,55^m$: A very strong aurora beneath some clouds in the north. It was all along the northern horizon, the lower part of the arch evidently

being below the horizon. No streamers. Later: The aurora seemed to have the summit of the dark part of the arch just at the horizon—that is, it could be seen not exceeding 1° high and for only a short arc.

April 5. $7^h 30^m$: A dull auroral arch in the north, which soon died out without becoming active. At $14^h 15^m$ there was no trace of aurora, but at $14^h 30^m$ there was a great, long, brilliant auroral cloud, say 60° long and averaging 10° broad, slanting a little to the west. It was one-third of the way up to the pole. This was very bright and somewhat irregular, smaller at the ends. The stars covered were seen through it. This rose slowly, and at about $14^h 50^m$ was half-way to the pole. A few minutes later it had disappeared, all but a faint smudge. The north end of the great cloud ended among the bright stars of *Cassiopeia*. There was no question as to its auroral nature. It was very bright, pearly white, even in moonlight. There were no other evidences of an aurora anywhere.

April 13. $13^h 20^m$: A strong auroral glow in the north. $14^h 0^m$: There were a few streamers to the west. No arch (arch below horizon?). Reported to be brighter just before dawn with distinct arch and a few streamers.

May 12. $10^h 5^m$: A strong auroral glow with feeble arch in the north. $13^h 20^m$: The aurora was very much brighter. The arch was at the horizon. It was shooting up streamers to the west. $13^h 30^m$: It had almost died out. There was only a widely diffused glow.

August 20. Bright arch and sheets of streamers. $14^h 55^m$: The sheets of streamers were in the northeast, at the northeast extremity of the arch. Arch very low and broad. The aurora died out soon and was faint before dawn.

September 11. 11^h: A strong auroral light in the north. $11^h 30^m$: An occasional streamer, but no arch. By 13^h it was very strong with very low arch and no streamers, but there were long sheets of pulsating light just above the arch. These would flare up for a few seconds and then entirely disappear. Toward dawn it seemed, if anything, to be brighter. The pulsations continued. Could only get out to see it once in a while, as I was photographing. It was reported to have been very active about 9^h with streamers, etc.

October 2. At $8^h 20^m$ saw a number of streamers shooting up from the northern horizon. Looked out again at $8^h 50^m$ and they were gone. Did not see any further evidences of aurora as late as when it clouded over at 14^h.

October 10. There seemed to be a very faint auroral glow at $11^h 0^m$. Mr. Sullivan also thought there was one.

October 13. $11^h 0^m$: A bright, active aurora, with arch almost on the horizon. It was shooting up streamers all along the arch. $12^h 30^m$: It was dying down. $13^h 0^m$: There was only a feeble glow in northern horizon. $16^h 30^m$: There was still a feeble auroral glow.

November 23 (R). $6^h 15^m$: Bright below. 10° wide.

December 10. $11^h 0^m$: A clear space in the north showed a feeble aurora was visible. This got higher until a very low uncertain arch was formed. This clear space remained for a long while. The aurora was brighter at midnight but

not active. By $13^h\ 0^m$ it had faded almost out. At $17^h\ 30^m$ it had disappeared entirely.

1908

January 8 (R). Aurora all night.

January 28 (R). Auroral arch.

March 20. $7^h\ 50^m$: A strong auroral glow in north and northeast. It was still visible at moonrise.

March 29. $11^h\ 15^m$: There were two pulsating masses at an altitude of 17°, one west, and the other east, of north. The west one was the bigger. It was in the bright stars of *Cassiopeia* and was 20° long by 5° broad. Its east end was elevated 10°. The west end was at the double cluster of *Perseus*. It became very bright and then faded out to almost invisibility. The fluctuations were irregular—sometimes the intervals were 5 or 6 seconds and at others 15 or 20 seconds. The east one was narrow and long and roughly parallel with the horizon. It was bright at times but later almost faded out. No other evidence of aurora. $11^h\ 30^m$: The pulsating clouds seemed to have disappeared. There were several faint streamers rising from the northern horizon as if the arch was below the horizon. $12^h\ 15^m$: All evidence of the aurora was gone. $13^h\ 30^m$: There was a pulsating cloud to the left of *Capella* about 5° above the northwest horizon. It would become very bright and then almost fade out. Mr. Sullivan saw it 15^m before and it was then very much nearer *Capella* and higher. It must have been moving west by south. $14^h\ 15^m$: There was no trace of aurora or pulsating clouds. Looking out several times after this before daylight there were no more evidences of aurora.

April 5. $15^h\ 40^m$: A low arch—only 3° or 4° high—with dark beneath, nearly due north. It was not bright.

May 1. $9^h\ 30^m$: A strong aurora, but no arch or streamers. Widespread auroral light in the north.

May 5. $12^h\ 0^m$: Through clouds a strong auroral light was seen in the north.

May 22. $9^h\ 15^m$: Quite a strong, but low, arch reaching half-way to *Cassiopeia*. $13^h\ 15^m$: The aurora was still bright but the sky was very thick. It died out about $14^h\ 0^m$.

May 23 (R). Aurora from $10^h\ 30^m$ to 13^h.

May 25. The display on this date was one of the most extraordinary that I have seen. The coincidence with a great electrical storm at this time was very singular, though it is probable that the two were not connected. At dark there were great banks of cumulus clouds in the west and northwest. These moved north and were alive with lightning—a most impressive sight.

A few minutes before $9^h\ 0^m$ a great number of narrow, but short, brightish strips like shreds of clouds appeared overhead. They formed a band across the sky from the north of west to the south of east through the zenith. The individual strips were moving west with great rapidity like low clouds driven by a heavy wind and appeared to be only a few hundred feet distant. From their luminosity it was at once evident that they were of an auroral nature. At this time there were

several great streamers in the north, but the northern horizon was covered with clouds. It was therefore not certain that any arch existed. In some ten or fifteen minutes the strips overhead, which were inclined sharply to the general direction of the band and which were $\frac{1}{2}°$ wide and $10°\pm$ long and had been moving westerly, had now blended into a beautiful, soft, but bright, band of cometary-looking light. This band was 10° wide and had its south edge in the zenith. It extended from the east by south horizon (in clouds) to the west by north where it disappeared in the great storm clouds. The sky was beautifully clear elsewhere. The band, though bright, was perfectly transparent to the light of the stars. At its ends it was apparently connected with two narrower and sharper and brighter cometary-looking strips that were perhaps connected with the horizon. The appearance was as if a great scroll had been flung across the sky and the end sticks to which it was attached had been dropped at an angle to the horizon. These end strips were at an angle of some 20° to the vertical, inclining to the south. At $9^h\ 20^m$, between the great arch and the pole, another band of short strips some 3° apart appeared. Near the meridian these strips pointed to the pole, while away from the meridian they pointed to the northwest. They apparently had no decided motion. These soon disappeared. The great band remained bright and uniform. It, however, was drifting very slowly to the south, and at $9^h\ 30^m$ it passed through the zenith. Then a parallel strip formed beside it and a few degrees north, which broke into short narrow strips pointing (overhead) toward the pole, but, beyond the zenith, to the northwest. These strips were longer and narrower away from overhead. They were not so well developed in the east. There was little or no motion in these. They fluctuated rather slowly in light and presently disappeared. Shortly after 10^h the great band broke up into short strips—at a sharp angle to its length—which had a rapid westward motion. Finally the band faded out overhead, the two ends becoming thus disconnected. The upper end of the west section drifted south so hat the two would not connect if continued. This kept very bright and became narrow, while the east portion split laterally into several parts. These shattered nto short strips 10° or 15° long which had a quick, short motion northerly. There were apparently other strips nearer to us that moved rapidly over the first ones. Their visible existence seemed to extend over about 10° of motion. About 10^h this easterly portion drifted overhead in a fragmentary form—frequently in this appeared the moving forms. At $10^h\ 15^m$ or $10^h\ 20^m$, at the time the masses of light in the zenith were fading out, the sky was blotted out by the storm clouds.

During all this time all over the west and northwest there was a magnificent and rapid electrical display in the clouds—so bright that it frequently blotted out everything else. Just before the storm closed in, a great, black, broad path of clouds came rapidly from the storm clouds in the west. This was perfectly straight at the edges and stretched from the northern to the southern horizons and was uniformly 15° or 20° broad with clear sky on each side. It swept overhead very rapidly—black, opaque, and sharply defined. As it passed I could see a few irregularities in it. These were moving very rapidly along its length to the

north, while the whole extent of the band itself moved rapidly a little to the north of east. When finally low in the east this object looked like an irregular strip of cloud. It was first seen near the west horizon under the great storm clouds and was then very black and sharply defined. There was no lightning in this band. I doubt if it had anything to do with the aurora, but its singular appearance and motion at this time were strange enough for record.

At $9^h 15^m$ the great zone of light stretched directly through the zenith. At the east it would strike the horizon 10° south of east. In the west it passed 10° north of *Venus*. Roughly it was perpendicular to the magnetic meridian. It was nearly 10° broad—bright and transparent. Its light strongly reminded one of that of a comet.

There were some newspaper accounts of this singular phenomenon, which seemed to have been seen at various other places. In general these accounts were of no value in locating the position in the sky. One newspaper had a long editorial on the subject and associated it with the zodiacal light, giving a lengthy history of the latter object. Of course there was no connection between the two phenomena.

According to Dr. Walter L. Rankin, of Carroll College, Waukesha, Wis., who observed the band, its summit was at an altitude of 70° and south of the zenith. Waukesha is 29 miles north and 13 miles east of the Yerkes Observatory. Taking into account the inclination of the band to the east-and-west line, his observation would make it about 140 miles high.

A description in the *Northfield* (Minn.) *News* says: "The peculiar aurora was in the shape of a long search light, like a path running east and west, from 7° to 10° wide, and at an elevation of 70° above the southern horizon." This would place it some 4 or 5 times as high as Mr. Rankin's observations. It seems quite probable that it was, in its later stages, several hundred miles above the earth's surface. The times were not given in any of the observations, and as it had a motion, southerly, of over 5° while under observation here, it is probable that the results given for its height are not very trustworthy.

I have thought that the remarkable nature of this display would warrant a full description, as some of the features were to me unique.

In *Science* for July 10, 1908 (**28**, 51), an account is given of what was apparently a similar display of the aurora on March 27, 1908, by Wilmot E. Ellis, of Fort Terry, N. Y. His confusion of this phenomenon with the zodiacal light is unwarranted. The zodiacal light never exhibits any such phenomena as the above. Indeed it does not show any marked changes which cannot be readily accounted for by the position of the observer or the atmospheric conditions at the time. The phenomenon of March 27 was purely auroral.

The night of March 27 was cloudy here, with a vivid electric storm and heavy rain in the first part.

May 26. $11^h 20^m$: There was a large, luminous, elongated cloud in the low northwest, whose light fluctuated rapidly. The entire sky was covered with patches and smears of luminous haze which I am sure were auroral. The night

was strangely luminous. The sky was apparently covered with a luminous patchy haze. In places it was streaky and sometimes long streams of it were seen. Some of these were nearly as bright as the brightest part of the Milky Way. With the lights out in the dome the windows looked as bright as if the sky were moonlit. With the exception of the pulsating cloud in the low northwest there were no other certain evidences of an aurora. But the unusual brightness of the sky and the patches and strips of luminous haze suggested that a strange auroral effect was on hand.

June 3. $11^h 30^m$: There was some attempt at streamers but no arch. $11^h 40^m$: Large streamers but no arch. There was a great deal of stray illumination. At midnight it began to darken near the northern horizon as if an arch were forming but it did not develop. At $13^h 40^m$ the aurora was dead.

June 4. No aurora during the night.

June 19. $10^h 10^m$: A faint arch with some feeble attempts at streamers.

July 29. Feeble aurora about $11^h 0^m$ for a short time. No arch or streamers.

July 31. At $14^h 0^m$, for a short time, there was the west part of a low arch which was quite strong, but which did not last long.

August 1. $12^h 30^m$: A very low arch and a few streamers. It soon died out and at $14^h 30^m$ there was nothing of it.

August 6. $12^h 0^m$: Rather strong aurora low on northern horizon. Arch either on horizon or below it. Did not last long—an hour or an hour and a half.

August 8 (R). Aurora.

August 18. $8^h 30^m$: A very bright aurora through breaks in clouds. There did not seem to be any arch. $8^h 40^m$: It was sending up streamers vertically under *Cassiopeia*. There was possibly a very low arch. The aurora was seen through breaks in clouds until moonrise.

August 19. $8^h 10^m$: A faint auroral glow low in the north. As late as $9^h 30^m$ it did not seem to be active.

August 20. $9^h 30^m$: An auroral glow in the north and a few streamers. By $10^h 30^m$ it had essentially died out.

August 21. A very bright aurora stretching far along the northeast and northwest horizons but not extending very high. It was very bright and active at $9^h 30^m$, sending up rays and broad sheets of light which were very bright at their bases—of a bluish-white color. The rays to the west of the magnetic north moved toward the west. $9^h 55^m$: Up to this time it was very active but now less so. $10^h 40^m$: Again active. $10^h 55^m$: It was dull but there were a few faint streamers. This was the brightest display I had seen for a long time.

August 22. There were no signs of aurora during the night.[1]

[1] At my request, during a short absence from the Observatory in August 1908, Professor D. W. Morehouse kept a lookout for auroras. The following record was received from him too late for insertion in the proper place. They are not taken into account in the table on p. 232:

1908 August 23: Faint glow. No streamers.
August 24: Strong glow. No streamers. Night very hazy.
August 25: A long, faint streamer a little west of north; no glow near horizon.
August 26: No evidence of aurora.

September 4. $7^h 30^m$: Bright aurora with nearly full moon. $9^h 0^m$: The arch was quite distinct. Its summit was about 20° to the right of the north point. $10^h 40^m$: The arch was very bright in spite of a bright moon. It seemed to have risen higher. There were fragments of a second arch above, three-fourths as high as the pole. Fragments of this ran over the *Pleiades*. There were some streamers to the left of the pole.

September 12. Mr. Lee reports that there was a large aurora after midnight last night (the 11th) through a very dense sky and nearly full moonlight. It was bright in the north with streamers and with fluctuating bright clouds overhead. It must have been a very large aurora, as the sky was too thick to see anything except the brightest stars. It appears to have been most active from 12^h to 13^h. There had been no aurora as late as 10 o'clock that night.

September 28. Mr. Sullivan reports that during a short period of clear sky at $14^h 30^m$ there was a brilliant aurora with some streamers.

September 29. $7^h 10^m$: Brilliant aurora seen through breaks in clouds, all over the north, northeast, and northwest. Some effort at streamers. $11^h 10^m$: The aurora had been very active with streamers and fluctuating patches of light. At about $8^h 30^m$ it was very brilliant. The sky in the north looked like daylight. The illumination extended a great distance east and west and the whole northern sky as high as the pole was bright, but there was not much activity. It got less bright and broke up into patches and became very active with streamers that reached higher than the pole. If any arch existed it was lost in the broken clouds which covered the sky more or less all the time. Patches or areas of light would ascend to great altitudes and then die out. At this time ($11^h 10^m$) the light had almost died out, but there were some streamers. The sky was pretty well covered with clouds. $12^h 0^m$: Some streamers and broken masses. Clouds. $13^h 0^m$: Less bright. Clouds.

September 30. Cloudy until 10^h. The clouds, disappearing, revealed a strong auroral glow. There was also a very small segment of an arch at the north by east horizon, which was not more than 1° high. The aurora was not active. By $10^h 30^m$ the arch had gone, but the glow continued. At 14^h there was some activity but no arch—only the glow and a few broad sheets of light shooting up from the horizon. The sky was very luminous all night—not haze, but like moonlight. I could read my watch in the Bruce dome at $14^h 0^m$ without artificial light.

October 1. At moonset a feeble auroral glow was seen in the north. This remained essentially all night without any activity. After midnight until dawn there was a pulsating, bright cloud about 25° north of east and 4° or 5° altitude. This would die out entirely for a while and then brighten up rapidly. About 16^h a brilliant bluish-white cloud 25° long by 6°–8° broad appeared midway between ζ and η *Ursae Majoris*. It was approximately horizontal. It would die out entirely and then in half a minute or so would rapidly brighten again, very bright, and remaining thus for a few seconds would die out again. This was a remarkably striking object.

October 2. The only evidence of an aurora was a fluctuating cloud 30° north of west and 5° high. This remained only a short time at about 14^h.

October 3. $11^h 30^m$: A strong auroral glow very low in the north. This remained more or less distinct until daylight. It was not active.

October 4. At moonset a feeble glow in the north. Then at about $15^h 30^m$ a large mass of pulsating light appeared due east at an altitude of about 30°. It was 15° long by 10° wide. It would almost fade out, then suddenly brighten. At times it appeared to be nearly double. It moved very slowly to the south and rather suddenly disappeared at $16^h 0^m$. It moved over about 10° in the three-fourths of an hour of its visibility. During the existence of this cloud the aurora in the north got very much brighter, but not active, and was fairly noticeable at daylight.

October 5. $14^h 0^m$: From this time on till daylight there was a feeble auroral glow. It was a little brighter at $15^h 0^m$.

October 12. $7^h 0^m$: Auroral glow began in low north—not there before this time. It got brighter rapidly and at $7^h 30^m$ was quite strong in moonlight and appeared to be forming an arch. $8^h 50^m$: Very active. Arch low and unfinished. Bright rays were moving rapidly to the left. It was conspicuous in spite of moonlight. $10^h 15^m$: It was apparently dead and had been so for half an hour or more.

October 30. Nothing in early part of night—had looked for aurora at moonset. First saw it at $13^h 20^m$. Not visible half an hour earlier. Very bright arch, almost like daylight. Then a dark under part came up, the whole rising slowly. At $14^h 0^m$ the arch was $6° \pm$ high and intensely bright. It soon broke and several streamers appeared. At $14^h 9^m$ the dark part disappeared and short streamers were visible all along the arch. These all moved to the right (east)—even those at the west end of the arch. $14^h 20^m$: Quiet—had faded much. $14^h 30^m$: Brighter with a great region of diffused light low in northwest. $15^h 0^m$: Arch formed again very bright but very low. Not active—dark below. $16^h 5^m$: Very bright again—active but no long streamers. The illumination was very wide east and west. $16^h 15^m$: Short streamers all along the arch. $16^h 45^m$: Still bright and active. There were no long streamers, only short ones and diffused masses of light moving east along the dark arch. The dark part would form very low, then rise to twice or more the original height. The light at times was so bright that it cast a shadow. Though the brightest aurora in a long time this was not a specially active one.

November 7. $10^h 0^m$: Full moon. Slender, bright auroral streamers shooting up from northern horizon to one-third or half-way to pole. No arch. The streamers came up from behind a low bank of cumulous clouds on the northern horizon. Ten minutes later no trace of the aurora could be seen nor was there any during the rest of the night. None was visible earlier than 10^h.

November 8. $10^h 0^m$: Full moon. Slender bright streamers shooting up from white cumulous clouds on the northern horizon. The aurora must have been of short duration, for it was not seen before this time nor after it as late as $15^h 0^m$.

November 16. $8^h\,30^m$: Strong auroral glow, very low, extending 5°–6° high. Ten or twenty minutes before this there was no trace of aurora. $9^h\,20^m$: Very bright and active, with bright short streamers. The streamers to the west of the summit moved to the left; those to the east moved to the right. $9^h\,50^m$: Very little left of it. $10^h\,10^m$: Only a glow left. $13^h\,0^m$: A slight glow still visible.

November 17. $6^h\,30^m$: Bright aurora. $10^h\pm$: Still visible with feeble arch 4° or 5° high—dark underneath and well defined. At $10^h\,20^m$ ϵ *Ursae Majoris* was exactly at the summit of the dark part—exactly on its highest edge. The aurora died out at $14^h\,0^m$.

November 18. $9^h\,0^m$: A rather large pulsating cloud in the low northwest, close to the horizon, and some light under the pole. (None there at $8^h\,0^m$.)

November 19. $12^h\,50^m$: Strong aurora behind clouds in the north. There was none in the earlier part of the night. $13^h\,45^m$: Still strong but not active.

November 28. An auroral glow low in the north for a short time about 12^h or 13^h.

December 4 (R). Aurora in evening and from 12^h to 14^h.

December 26. Apparently no aurora (and none for a long time). The sky, however, was very luminous. I have often noticed this luminosity of the sky. Though it appeared to be perfectly clear on this night it was whitish all over, as if the sky were phosphorescent.

1909

January 1. $15^h\,40^m$: Streamers from behind clouds in the north—the first aurora I had seen for a long time. It seemed to be quite active.

January 24. $12^h\,0^m$: A bright aurora all along the northern horizon among broken clouds. Not active. First saw it at $11^h\,45^m$—it was not visible an hour earlier. $13^h\,0^m$: Aurora visible as a bright streak along the horizon under the clouds. $14^h\,0^m$: Same as before. $15^h\,0^m$: Was still visible under the clouds.

January 25. $10^h\,0^m$: Auroral glow in the low north. $10^h\,30^m$: Only feebly seen. $12^h\,40^m$: It was active. If any arch existed it was below the horizon. There was not even a glow along the horizon but the streamers and sheets of light rose nearly half-way to the pole. They were not bright.

January 26. $15^h\,0^m$: Faint aurora, with very low arch. There was none an hour earlier. $16^h\,45^m$: Arch very low, 3° high. No streamers. Bright changing patches in the arch.

February 21. $9^h\,30^m$: Very low arch. Dark part 3° high; top of bright arch 5°–6°. $10^h\,30^m$: There was an auroral arch at the horizon, the dark part having sunk below. $16^h\,0^m$: The aurora had died down. There were attempts at streamers for a while. $16^h\,30^m$: It was very bright again. Arch almost on the horizon—so low that there was scarcely any dark beneath.

March 19. There did not seem to be any aurora until $8^h\,45^m$, when a long streamer appeared east of north which moved west. At this time there was only the feeblest glow and no arch. It became more active later and the glow was

stronger. $9^h\ 15^m$: Many streamers but no arch. $9^h\ 45^m$: Great deal of glow and streamers some of which reached nearly to the pole. $11^h\ 0^m$: The aurora was dead. $12^h\ 30^m$: Nothing visible. If there was any arch on this night it was below the horizon.

March 20. $12^h\ 45^m$: Considerable auroral light and a few streamers. (R) says "all night."

March 21. $7^h\ 50^m$: A feeble auroral glow.

March 28. $11^h\ 30^m$: Splendid aurora; curtains; ascending pulsations. The curtain waves went east. At first the narrow streams went west. $12^h\ 20^m$: It was not active at this time but the arch was very bright. It extended very far east. $12^h\ 25^m$: It was almost dead. Same at $13^h\ 20^m$. This aurora was the finest example of curtain effects that I have seen here. They were white and some of them bluish white. These curtains moved rather rapidly east while the slender streamers that made their appearance previous to the curtains moved west all along the arch. There were rapid ascending pulsations, and a broken second arch above the main arch. $15^h\ 40^m$: Aurora active again. No definite arch, but masses resembling bright cumulous clouds from which sheets of faint light were going up. The aurora was more or less active until daylight. It would frequently almost die out, then brighten up again. Its most active period was before midnight—about $11^h\ 30^m$.

Observer absent from April 19 to April 30. Mr. Lee reported that there was an aurora with low arch and some streamers at about 15^h on April 25. It was not bright.

May 15. Cloudy in first part of night. $11^h\ 30^m$: A brilliant fluctuating cloud in about $\alpha\ 9^h\ 0^m$, $\delta+47°$. This would entirely disappear and brilliantly reappear again. It was irregular and extended easterly for 20° or 25°. At the same time a very small cloud was at the horizon in the northwest. This would flare up very bright and then fade again. In 10^m or 15^m these fluctuating clouds went out entirely and disappeared. There was no auroral effect elsewhere, except possibly a very uncertain and feeble glow in the north. This glow became stronger, but no arch. $12^h\ 35^m$: The brilliant pulsating cloud appeared again, or another like it. It passed through the bowl of the Great Dipper and extended through *β Ursae Minoris*. The small cloud was very bright at the northwest horizon. It was 5° high (just under the tip of the sickle of *Leo*). At $13^h\ 5^m$ they were fluctuating brilliantly. The large elongated mass passed through *η Ursae Majoris* and *α Draconis* and beyond, involving *Vega* on its south side. The whole mass moved east and south. $13^h\ 35^m$: A small cloud 3° or 4° in diameter appeared in the southeast at an altitude of 5° vertically under *ε Pegasi*. It would get intensely bright for a few seconds and then fade out. The large mass moved through the zenith and over *γ Cygni*. Another mass 45° high in the east. They all would flare up intensely bright. $14^h\ 40^m$: The small cloud in the east was rising and moving slightly north. A magnificent mass extending east and west near the zenith with its east end north of the small cloud by 5° or 6°. It was very long and near the middle jutted out to the south. It would become

intensely brilliant. The large one lay approximately east and west. $14^h 20^m$: The east ones had disappeared while an intensely brilliant one, 45° altitude, was fluctuating in the west. It was very large and brilliant, though seen through haze. Sky in north luminous but no streamers or arch. This was one of the finest displays of these fluctuating clouds that I have seen.

May 17. $11^h 25^m$: A small spot of fluctuating light in the east half-way from the Dolphin to the horizon. It was not bright and did not last long. A large but faint glow in the north, but no arch. $11^h 53^m$: One of the fluctuating clouds appeared in the west. It was low, and about 4° in diameter. It was 4° to the right of ϵ *Leonis*. By this time the glow in the north was much stronger and the bright mass in the east had disappeared. $12^h 0^m$: Arch very low and flat and very extended, close to northern horizon with momentary efforts to produce a second arch above and some effort at streamers. A great, long, fluctuating, bright cloud passing through η *Ursae Majoris* and overhead. Though there were no streamers the aurora seemed to be getting active. $12^h 5^m$: A large fluctuating cloud in the Milky Way in *Cepheus*—between *Cygnus* and *Cassiopeia*. It would become very bright for moments. A fainter one 20° to the left of the polar star. $12^h 30^m$: Very bright everywhere under the pole, with some attempts at streamers. Bright arch, very low and very extended.

May 18. $9^h 30^m$: Strong aurora in the north below the clouds. It kept bright more or less all night, though partly covered with clouds, but less bright after midnight.

June 22. $11^h 10^m$: Slight aurora—the first in quite a long time. Watched every clear night but there was none. $14^h 0^m$: Very bright through break in clouds.

July 23. $9^h 30^m$: A rather feeble arch, which sent up streamers earlier. $10^h 20^m$: Much stronger. $10^h 38^m$: Arch gone. $11^h 50^m$: Arch very strong. $13^h 0^m$: Very strong glow but no arch. $13^h 35^m$: Glow still strong, no activity. $14^h 20^m$: Pretty dense glow. $14^h 40^m$: Aurora dead.

August 19. $12^h 0^m$: Small aurora which soon ended.

August 28. $14^h 30^m$: Strong auroral arch (moon near setting) but not very definite. $14^h 50^m$: Arch very strong and thin and definite. 5° high, very dark below it. η *Ursae Majoris* seen in the dark region. No streamers. $15^h 10^m$: The definite black beneath the arch had disappeared. There was at this time a broad bright band 7° high. Later on there was a very little dark space close to the horizon. The band became irregular—broken along its length—when daylight came on.

September 5. $8^h 15^m$: Slight aurora low in the north beneath the clouds. It did not last long.

September 14. $13^h 0^m$: A small aurora.

September 25. $15^h 30^m$ to $16^h 0^m$: There was a rather strong auroral arch low in the north. No streamers.

October 4. $8^h 30^m$: A bright pulsating cloud in the Milky Way, α $18^h 20^m$, $\delta - 18°$. It would become very bright for a few seconds, then die out. It remained visible for about 10 minutes. No other evidence of an aurora.

October 7. $10^h 25^m$: A strong low arch very dark beneath. It was not present, say 15 minutes before. $11^h 0^m$: The summit of the arch was in the same vertical with *β Ursae Majoris*. The top of the dark arch was half-way from the horizon to this star. The top of the bright arch extended three-quarters of the way from horizon to *β*. $11^h 20^m$: Arch strong, but no streamers. $11^h 30^m$: No change.

October 8. $6^h 30^m$: Feeble aurora. By $9^h 0^m$ it was dead.

October 18. A magnificent aurora was visible at 7^h. I first saw it at $7^h 45^m$. At this time all the northern sky below the pole was covered with great masses of light, with apparently a double arch. It was very active at $8^h 0^m$, with streamers and broken masses of light. At $8^h 25^m$ there were great masses of light far in the northeast which drifted slowly to the west and rose higher until the sky to the zenith and as far as *Saturn* was covered with them. At $8^h 30^m$ there were rapid ascending pulsations. These ascending waves seemed to bring out and illuminate large areas of matter all over the north, that were drifting westward. At $8^h 35^m$ the arch had again formed and streamers (with dark intervals between them) all along its periphery. A very fine sight. Beautiful slender streamers ascended as high as *Polaris*. Several of those to the left moved rapidly to the right or eastward. These apparently passed others going west. To the west the streamers were reddish, but the general display was of a bright yellow. Many of the slender streamers shot higher than *Polaris*. At $8^h 45^m$ all the northern sky was covered with tufty masses of light which were elongated vertically. At $9^h 5^m$ a thin bright arch extended to an altitude of 10°. At $9^h 45^m$ there was a fine and bright arch at an altitude of 14°, with bright masses far toward the east apparently separated from the rest of the aurora. *α Ursae Majoris* was in the upper edge of the bright arch, which was black beneath only toward the east, the rest being luminous below the arch. The arch was very perfect but not active. At $9^h 58^m$ rapid waves were ascending all along the arch and pulsating diffusions of luminosity all over the heavens north of the zenith, but the arch was not active, no streamers. At $10^h 15^m$ the arch became active to the west. It had risen slowly and was then 17° high, but it was not dark underneath. Waves of faint light were ascending everywhere in the north, to the zenith. *α Ursae Majoris* was at this time in the lower part of the arch. There were masses of light in the east free of the arch which was very large and wide, extending along the horizon for 100°. At $10^h 22^m$ there were beautiful curtains in the west, springing from the arch and extending to great altitudes. These moved rapidly to the east. Their bases were bluish white with strong prismatic colors. It was a splendid sight! Beginning at the west these curtain-forming streamers would burst out along the summit of the arch, which was very perfectly formed, one at a time in rapid succession ascending to great altitudes. Thus, the arch was broken to pieces. The whole system of moving curtains moved bodily to the east. This greatest display lasted only a minute or two, and was one of the grandest I have ever seen. Then everything broke up into masses of light all over the north to the zenith. During this display there was a great curtain-like mass below the middle of the

arch which nearly touched the horizon and moved to the left. $10^h 30^m$: Great streamers everywhere in the north reaching nearly to the zenith; very active. Great masses of light and fragments of streamers all over the north, but no arch. At $10^h 35^m$ rapidly ascending waves were rising everywhere in the north with great masses of light all over the northern sky extending to the east point. $10^h 47^m$: Fantastic masses dancing all over the northern heavens to the zenith, which seemed to be due to rapid pulsations of light, which, as they passed, momentarily illuminated irregular masses of matter that were changing form slowly and moving to the west. $10^h 52^m$: The rapid pulsations seemed to have ceased, but there were still great masses of light all over the north to the zenith, which would slowly brighten and fade. At this time the arch was again forming. At $10^h 55^m$ the arch was very black below and only half as high as before. Ascending pulsations and bright masses all over the north to the zenith. The sky was more or less luminous everywhere except in the far south. The pulsations were still rising at $11^h 5^m$ and the arch was broken, but at $11^h 35^m$ it was very strong again and very dark underneath. Ten minutes later it was broken again by masses of light and some streamers. At $13^h 0^m$ the arch was still pretty strong, but not active. At $16^h 20^m$ it was all dead except some pulsating masses of light near the northern horizon. $17^h 0^m$: The same.

This was one of the finest auroras I have seen here. Especially was the display of curtains splendid at $10^h 22^m$. I have never before seen the brilliant prismatic color effects which burst out on the forming of the curtains. I was strongly impressed with the resemblance of some of the streamers to comets' tails, etc. They would shoot up, sometimes very slender, and then diffuse into broad wavy masses moving west. The general motion was west except the curtains mentioned and the few streamers that moved east, and the pulsations which were vertical. At times great areas of feeble illumination would appear for a second or so all over the north. Sometimes these illuminations were very bright. They appeared like great areas of bright haze. Altogether the display was one of the most brilliant I have ever seen.

October 19. $8^h 45^m$: A long bright mass in the Great Dipper which was $20° \pm$ in length. West of this was a smaller mass which would brighten and fade. In the east was a long luminous region 20° high and south of the east point. The entire sky was luminous as if a considerable moon were shining. No arch. The long mass in the Dipper seemed to be a part of an arch. $10^h 30^m$: It was sending up quite a number of streamers, but not bright. There was no arch. The streamers came up from the horizon. The pulsating clouds were gone. $11^h 20^m$: It was dead. $12^h 0^m$: The sky was luminous as if a bright moon were shining, but no trace of aurora.

October 26. $17^h 40^m$: After moonset there were two pulsating clouds, one under the pole $10° \pm$ altitude, the other farther to the east.

November 14. $14^h 30^m$: Very low arch, 2° high—rather faint, thin, and not over 25° in extent. Not there half an hour before. $14^h 50^m$: Arch very much brighter and had risen somewhat. It lasted until dawn but was not active.

November 19. $10^h 30^m$: After moonset a strong auroral glow in the north. No arch. $14^h 0^m$: Glow was still present among clouds.

December 9. $15^h 0^m$: Part of an auroral arch to the left of the north point.

REMARKS ON THE RESULTS OF THE OBSERVATIONS

In reading the accounts given in *Nature* and the *Astronomische Nachrichten* of the luminous nights seen in England and on the continent about the first of July 1908, by Denning and others, I have thought that the phenomenon of the luminous night of May 26, 1908 (and of other dates), was of a similar nature to those described by the various observers in July.

In *Nature* for September 30, 1909 (**81**, 395), Dr. Chree gives an account of a great magnetic storm, recorded at Kew, which "commenced suddenly at about $11^h 43^m$ A.M." on September 25: "The storm was of comparatively short duration, no movements of any great size being recorded after $8^h 30^m$ P.M. on September 25, and by 1 A.M. on September 26 little trace of the disturbance was left."

This same storm badly interfered with telegraphic operations all over the United States on September 25. According to the newspapers it was very active between 6 and 9 A.M. I observed all night on the 24th, closing observations just before 17^h or 5 A.M. of the 25th, or 11 A.M. at Greenwich. I looked out for any aurora just before daylight but there was none, nor had there been any during the night which would be bright enough to be seen in moonlight. As recorded above in my notes, there was an aurora on the 25th at $15^h 30^m$. I had suspected one earlier in the night but it was uncertain on account of the moon.

For a valuable account of the condition of the solar activities about the period of this great disturbance, see an article by Professor Frederick Slocum in the *Astrophysical Journal* for January 1910.

For some years, I have had in my mind a scheme for systematic observations of the aurora, but for various reasons it has not been possible to carry it out, as it would require the permanent residence of one observer some miles north of here. The scheme would be to have someone, say a resident of some place ten or twenty miles north of the Yerkes Observatory, who would be familiar enough with the

stars to locate an object with the naked eye by them, or who had an instrument for the determination of the altitude and azimuth of such objects. Suppose such an observer to be connected with the Yerkes Observatory by telephone. Upon the appearance of any striking auroral phenomena, such as the moving, fluctuating clouds, simultaneous observations could be made of these that would give their true altitude above the earth. As they sometimes remain visible for upward of an hour, these observations would show if they varied their height and would also give accurately their real velocity with respect to the earth. The actual elevation of the arch could thus be determined also, and various other phenomena of importance. A simple instrument made of wood, such as I have described in the *Astrophysical Journal*, **16**, 144, 1902, would give the position of the object with sufficient exactness for the purpose—especially if pointings were also made on some known star at the time. Observations of this kind would quite definitely show whether different observers at a distance really see the same thing or that each person sees his own aurora, as in the case of the rainbow, as has been suggested by some writers. The fluctuating clouds and their motion would alone perhaps contradict this theory.

In the present paper, I have gone somewhat into detail in the accounts of the auroral displays here. This has been done because it has appeared to me that in general the references to an aurora usually are wanting in details and leave doubt as to what kind of aurora had been present. It would seem that there may be different kinds of auroras due to different causes or to modifications of some one cause. If so, these should be distinguished from each other. For instance, sometimes the fluctuating clouds have appeared when there was no other evidence of an aurora. At other times they have been present during an ordinary aurora. These clouds therefore would seem to depend on different conditions from the regular aurora. At times these conditions alone are present, at other times they combine with those necessary for the production of the ordinary aurora. Certainly the conditions must have been different that produced the great band of May 26, 1908. So also must those that produced the luminous nights mentioned in these observations. A classification of the various kinds of auroras would therefore be valu-

able. It is with this idea in view that I have dealt more extensively with some of the displays of the past ten years.

The recorded times are important, especially in case of the spasms of activity in the large auroras. It would be interesting to compare these with the magnetic records to see if any special disturbances show at these times on the instrumental records.

I will not here go into any discussion of the connection between solar disturbances (as indicated alone by great sun-spots) and the aurora. This does not lie within my province. It may not be out of the way to state, however, that such a connection does not at present seem to be clearly established in all cases. I have within the past ten years or so frequently noted solar spots so large as to be visible to the unaided eye. These have not always been closely associated with auroral displays. A most striking instance of this kind was shown in the case of a large naked-eye sun-spot on and about December 29, 1909. A careful record on every clear night about this time failed to show any evidence of aurora. Indeed this prolonged absence of auroras (up to the latter part of January 1910) would have been noticeable without the incentive of the large sun-spot to look for them.

I have thought that it might be interesting to see if any months are more abundant in auroras here than others. For this purpose Miss Calvert has prepared from my observations the following table, which is interesting from several points of view.

TABLE OF AURORAS OBSERVED FROM 1897 TO 1909 INCLUSIVE

	1897	1898	1899	1900	1901	1902	1903	1904	1905	1906	1907	1908	1909	No. by Months
January	..	2	1	..	..	..	..	..	..	..	..	2	4	9
February	..	..	2	..	..	..	..	..	2	2	12	..	1	19
March	1	6	..	..	..	..	..	..	2	1	3	2	4	19
April	1	..	..	1	..	1	..	2	..	..	2	1	1	9
May	..	1	3	1	..	1	..	..	..	1	1	5	3	16
June	..	..	3	..	..	..	..	2	..	1	..	2	1	9
July	1	..	1	..	..	..	..	..	..	..	..	2	1	5
August	2	1	1	..	..	..	1	..	..	1	1	7	3	17
September	..	4	4	1	..	..	4	..	1	2	1	5	3	25
October	1	1	..	..	..	..	1	3	..	1	3	7	..	17
November	..	2	1	..	1	..	4	3	1	..	1	6	..	19
December	2	1	..	..	..	..	1	..	..	..	1	1	..	6
No. by yrs.	8	18	16	3	1	2	11	10	6	9	25	40	21	170 = total

This table gives the number of auroras seen in each month from 1897 to the end of 1909. The last vertical column gives the entire number for each month during this interval. The horizontal column at the bottom of the table gives the frequency for the various years. September has been especially prolific, but September is a season of clear skies. February also stands high, but this high grade is dependent on the year 1907. July and December stand especially low. There seems to have been a minimum of auroras from 1900 to 1902. The observer was absent on the eclipse expedition to Sumatra from the first of January to the last of July of 1901. The year 1908 gives the highest record of all—auroras on 40 nights. But the summer and fall of 1908 were remarkably free from clouds. The combination of 1907, 1908, and 1909 would seem, however, to indicate a maximum in which we are now placed or which we have just passed through.

It is evident, however, that to determine accurately the frequency of auroras here in the past thirteen years it would be necessary to compare the observations with the various meteorological conditions that have prevailed in that time. I have not the leisure at present to undertake this work and so the results must stand as they are.

YERKES OBSERVATORY
January 1, 1910

STAR COLORS. A STUDY IN PHYSIOLOGICAL OPTICS

By LOUIS BELL

The following investigation is an attempt to discover the relation between the visible colors of the stars with the known facts regarding their spectra, and the varied and bizarre array of tints reported by many of the observers of double stars. The difference between the sober evidence of the spectrograph and visual observations of isolated stars is very small compared with the recorded observations of double stars made even by experienced observers with entirely adequate apparatus. There is sufficient kinship in the various estimates of color thus made to indicate dominant common factors, whether objective or subjective in their nature, and yet the differences and discrepancies are very singular and are sufficient to have led to somewhat startling hypotheses as to great accidental or periodical variations in color.

To begin with, a most casual study of the heavens convinces the observer that among the lucid stars at least there are no extraordinary colors. Broadly speaking, they range from a faintly bluish white, like *α Lyrae*, to a fiery orange, like *α Scorpii*. It is well known that there are at least no isolated stars which can fairly be called blue or green, or in fact which are characterized by any strong dominant hue in the more refrangible part of the spectrum. Even *β Librae*, occasionally reputed to be green, presents to the normal eye only a very faint and faded tint. The spectra of the stars with respect to the corresponding colors have been thoroughly investigated in the Potsdam *Durchmusterung*, as discussed by Pickering,[1] as well as earlier by Vogel,[2] and by Krüger.[3] The results obtained in these investigations are entirely consistent with the evidence of the eye so far as isolated stars are concerned. The stars of the Potsdam list are recorded in a systematic scale ranging from white through the various shades to yellowish red. There is no record of colors materially outside this

[1] *Harvard Annals*, **64**, 125 ff.

[2] *Publicationen des Astroph. Obs. zu Potsdam*, **3**, 127, 1883.

[3] *Publicationen der Sternwarte in Kiel*, **8**, 1893.

simple and commonplace range. The careful analysis of the Potsdam colors by Pickering shows how closely the visible appearance and the spectra correspond. Using the spectral classification of the Draper catalogue it proved possible in the case of the Potsdam observations to make a singularly orderly classification of the spectra with respect to color. The Potsdam list, to magnitude 6.5, included 4206 stars. From the analysis of the data relating to these it plainly appears that the visible colors can be fairly well predicted from the photographic spectra. For example, the chances are 15 to 1 that a class A star is either white or yellowish white, and the chances are 150 to 1 that it will not be called a full yellow. Likewise the late solar stars of classes G and K are predominantly whitish yellow or yellow. The chances are 8 to 1 that a star of these classes is not either white or yellowish white, but rather of a deeper tint, and the chances are 200 to 1 that a star of these classes does nor fall into the white category, these figures being derived from the color estimates of the stars of different spectral classes in the Potsdam list. Moreover, Pickering[1] has shown that there appears to be no evidence that stars of the same class of spectrum differ in color in different parts of the sky.

In considering the fainter stars of the Potsdam list, as in other similar records, there is an evident increase of the apparently white stars of the fainter magnitudes, say from magnitude 6 to 9.6, and of the total number of stars classified as white to magnitude 6.5, 94 per cent. are of classes B or A. The increase in the number of apparently white stars among the lesser lights is not without physiological significance, as Müller and Kempf[2] have shown. From the spectroscopic evidence it is perfectly clear that, so far as isolated stars at least are concerned, the spectra correspond to the visible colors with remarkable closeness, and neither the eye nor the spectroscope gives evidence of unusual colors in isolated stars, especially noteworthy being the complete absence of anything approaching strong blue or green. On the other hand, the reputed colors attributed to many double stars are of the most eccentric and extraordinary character compared with anything revealed by the spectroscope or to the eye in ordinary stellar observations.

[1] *Loc. cit.*

[2] *Astronomische Nachrichten*, **180**, 16, 1909.

The following is a brief list taken quite casually from Webb's *Celestial Objects*, exhibiting the vagaries of nature or of vision in double star chromatics: Indigo, grayish white, olive, bluish gray, bluish red, violet, tawny, ruddy or dusky, brownish, pale rose, fawn color, olive blue, greenish blue, pale green, yellowish green, green, rosy, gray, lilac, mauve, bluish green, ashy yellow, bright green, greenish blue, pale tawny, sombre, cool gray green, greenish red, ruddy olive, dusky red, garnet. These thirty reputed hues are only a part of the eccentricities of color nomenclature which appear in the literature of double stars. A complete list of such fanciful epithets would run to nearly a hundred, but those given are sufficiently characteristic and so obviously outside the range of normal star colors as to demand investigation. From the physical and spectroscopic standpoint such a list is quite inexplicable. From the physiological standpoint it turns out to be quite in accordance with the well-known phenomena of physiological optics.

The very striking fact of the predominance of blue, violet, and similarly colored *comites* among double stars in which apparent color differences exist becomes thus comparatively easy of explanation, together with the remarkable fact that where one star of a pair is blue it is invariably the smaller one. The elder Struve[1] made a careful examination of the colors of double stars and among 596 comparatively bright pairs found 120 blue *comites*, these being all the *comites* in which there was strongly marked color differing from that of the primary. These doubles included both binary stars and optical doubles without discrimination. Later lists give a still greater number of blue *comites* to be accounted for. Levander,[2] for example, in his analysis of various star lists gives 394 *comites* of blue, green, lilac, purple, and violet tints. As his list was compiled very largely from Webb's *Celestial Objects*, the other sources being mainly catalogues of red stars, these 394 are all, or nearly all, the smaller components of doubles. Holden[3] states that stars of such colors are, so far as he knows, invariably associated with larger stars, the colored ones in general being small. Of Levander's list, just cited, 369 of the

[1] *Mensurae Micrometricae.*

[2] *Monthly Notices*, **50**, 33, 1889.

[3] *American Journal of Science*, **19**, 467, 1880.

394 are of the seventh magnitude and below. One is confronted then with the extraordinary fact that of nearly 400 blue and similar stars all are the smaller components of doubles, most, or all, of such colors ascribed to isolated stars being either exceedingly pale, as in the case of *a Lyrae*, or erroneously described, as indicated by Holden.[1] Now the chance of all blue stars being associated, as they are within an average distance of one-half a minute of arc or less, with brighter stars of different color is on the doctrine of chances so small as to be practically negligible. Inasmuch as there are only about 300,000 stars in the northern hemisphere of magnitudes including the larger members of the colored pairs, and these are scattered over 20,626 square degrees, the chance of a fortuitous distribution of 300 or 400 colored stars all forming doubles with brighter primaries needs no discussion. One is therefore driven to an explanation of the situation dependent in some way or other upon propinquity. It has been a hypothesis many times considered that the association of a blue *comes* with a differently colored primary is due in some unexplained way to the physical connection which may exist between them, and so little is known of the actual physical conditions existing in binary stars that such a hypothesis is on its face readily admissible. If such be the explanation, that is, that the color difference depends upon physical connection, then one should find it a peculiarity of stars which are gravitationally related, that is, of the binary class, and it should not exist among stars known not to have a binary relation but to be merely optically double. Holden[2] examined from this point of view and with great care a list prepared by Burnham of all the known binaries at the date of his paper. This list, after excluding two or three dubious cases, contained 162 known binaries of which 40 showed color differences practically all of the nature here considered, or 24.7 per cent. of the whole number. Now Flammarion[3] had a few years before made a similar investigation regarding double stars known not to be binaries, but showing from their proper motions that the connection between them was merely optical. If the color difference is due to physical connection only, it should

[1] *Loc. cit.*

[2] *Loc. cit.*

[3] *Comptes Rendus*, **87**, 835, 872, 1878.

not appear among the stars of Flammarion's list. On the contrary, it does appear in 32 doubles out of 131, or 24.4 per cent. In other words, the proportion of colored doubles is substantially the same whether one considers binaries or optical doubles, and this proportion also holds very closely for Struve's list made up of a considerably larger number of doubles without regard to their physical connection. The lists of Holden and Flammarion contain a few doubles of which the *comites*, while different in color, differ very slightly, and casting out these so as to leave those distinctly of bluish or similar hue, the percentages in the lists of Holden and Flammarion fall respectively to 21 and 20.9 per cent. as compared with Struve's 20.1 per cent. So close agreement is doubtless accidental, but the figures are sufficient to show that there is absolutely no material difference between doubles in general, binaries, and known optical doubles, with respect to the frequency of occurrence of bluish *comites*. Flammarion suggested that his paper gave evidence that isolated stars were sometimes colored, blue and green, but clearly the theory of probabilities renders the chance of Flammarion's colored *comites* having been associated with their primaries by purely fortuitous propinquity, in the absence of any other isolated colored stars, one that is quite negligible. It therefore appears that the existence of colored *comites* must be referred to causes connected with optical propinquity and not dependent on physical connection.

A somewhat significant fact in this connection is the very small proportion of doubles with bluish *comites*, of which the primaries are white. In Holden's list of binaries but five out of forty colored pairs have white primaries. In Flammarion's list of optical doubles the proportion is but 6 to 32, while in Struve's list there are but 53 white primaries out of 173. Primaries of bluish *comites* run predominantly to various shades of yellow and orange in spite of the fact that in general, and especially in the fainter magnitudes, among which most doubles are found, the proportion of distinctly yellowish stars is decidedly small. Fig. 1 shows graphically the variations of apparent star color with magnitude as derived from Müller and Kempf's discussion of the colors of the Potsdam *Durchmusterung*.[1] The colors used here for a general classification are white, yellowish

[1] *Loc. cit.*

white, whitish yellow, and yellow, being a condensation of the general Potsdam color scheme. Müller and Kempf call attention to the difficulty of recognizing colors under faint illumination and to the difference between large and small telescopes in the general color effects. This is a natural result of the theory of color vision, to be referred to later on, and it in fact shows very plainly in the curves, the blanching of the smaller stars, below magnitude 6.5, being very marked. The increase in the number of whitish stars is very apparently at the expense of the deeper-colored classes. The subjective

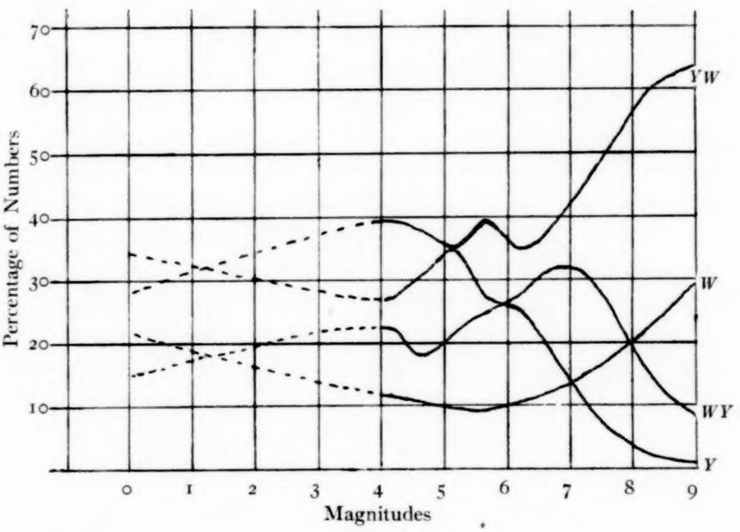

FIG. 1

nature of the phenomenon is borne out by the fact that in the vicinity of the ninth magnitude nearly 90 per cent. of the stars are whitish, at least as seen with moderate apertures, while so far as the spectra of the fainter stars have been investigated, this whitening would not be sufficiently accounted for by increase in the proportion of the type I stars observed. The subjective element in these estimates of star color, or rather the physiological element depending on both instruments and personal equation, is rather conspicuous in spite of the fact that the observations of a single skilful observer, or a group of observers working under uniform conditions, are sometimes remarkably

consistent. Table I shows the percentages of stars of various colors to magnitude 6.5, as shown in a very interesting paper of Franks,[1] and in the Potsdam colors as analyzed by Pickering,[2] respectively.

TABLE I

Franks: Color	Per cent.	Potsdam: Color	Per cent
White	31	White	13.2
Yellowish white	20.8	Yellowish white	40.2
Pale yellow	22.4	Whitish yellow	23.0
Yellow	11.8	Yellow	20.6
Pale orange	7.7	Reddish yellow	2.98
Orange	5.6	Yellowish red	0.02
Orange red	0.7		
	100.0		100.00

The Potsdam observers saw fewer white and fewer reddish stars than did Franks and evidently classified as yellowish white many stars which Franks called white. Franks's table is based on 3497 stars of his own manuscript catalogue and the Potsdam observations on 4206 of the Potsdam colors. In spite of such differences the infrequency of white or whitish stars and the frequency of those strongly yellowish in the lists of colored doubles are very striking.

The relation of color to difference in magnitude in doubles is also striking. It was noted by Struve[3] many years ago in his analysis of his own catalogue of doubles. He found an average difference of about one-half a magnitude for doubles of exactly the same colors, of something over a magnitude of those possessing slightly different colors, and of over two magnitudes of those showing distinctly different colors. Holden's list of binaries discloses a difference of 0.53 magnitude for binaries of the same color and 2.44 for binaries of different colors. Flammarion's list of optical doubles is somewhat difficult to compare, owing to the fact that the optical doubles are very generally much wider apart than the binaries, including some separated by several minutes of arc. Flammarion's list of colored optical doubles shows an average difference of magnitude of about 2.5, excluding very faint and wide *comites*, a few tenths less including everything. His list of optical doubles of the same color, or nearly

[1] *Monthly Notices*, **68**, 672, 1908.

[2] *Loc. cit.* [3] *Loc. cit.*

the same color, shows difference of magnitude of 1.5, excluding very wide doubles (several minutes of arc apart) and those with *comites* of tenth magnitude or below. Including everything the proportion in the two classes does not differ widely. The fact that nearly all stars of Holden's list are also in Struve's catalogue, which also includes many of Flammarion's list, indicates the same general relation of difference of magnitude to difference of color both in stars strictly binary and in those at large.

The upshot of the statistics in the matter is that most doubles having bluish *comites* have a conspicuously greater difference in magnitude than doubles of the same color, that the former have very notably a large proportion of yellowish primaries, and that binary stars show no greater tendency toward such coloration of the *comites* than do double stars at large. The cause of such coloration must be sought therefore outside of matters involving physical connection as already noted, and this investigation is directed toward assigning the definite physiological causes, which produce, in stars in optical propinquity and differing in magnitude, the particular kinds of apparent coloration that have been persistently attributed to colored doubles. If there were available for a long list of double stars the spectra of both components, one could easily evaluate the objective factor in these colorations so far as it exists. The number of stars thus completely investigated is, however, very small, some important factor in the case being often absent even when careful spectrographic records have been obtained. The writer has been able to find 25 complete records, however, among the stars of the Harvard Observatory catalogue, which are shown in Table II. Ten of these show doubles having *comites* of the same spectral type as the primary, in 9 cases of type I. The striking thing about the list is the very small color difference which exists in any of the cases cited. In all the cases where there is anything that might be called a noticeable difference of color, the colors themselves have been noted as somewhat variable, and in no instance is there a really considerable difference of magnitude. The particular significance of this table will appear later in the discussion of artificial stars. The second section of the table also contains 10 stars, all of which exhibit violent contrast, and each star of this list shows its *comes* of earlier spectral type than the

TABLE II

DOUBLES WITH *Comites* OF THE SAME SPECTRAL TYPES

Star	Color A	Spectrum A	Color B	Spectrum B	Mag. A	Mag. B	Remarks
α Geminorum	Greenish white	I	Greenish white inclining to yellowish	I	1.99	2.85	B of slightly later class
ν Scorpii	Bright white	I	Pale lilac	I	4.29	6.49	Some variation as to colors
θ Serpentis	Yellowish white	I	Yellowish white	I	4.50	5.37	A5, A5
α Piscium	Whitish	I	Bluish cast	I	4.33	5.23	
κ Boötis	Greenish	I	Bluish	I	4.60	6.61	A5, A5 ?
ζ Ursae Majoris	White	I	White	I	2.31	3.85	
100 Herculis	Greenish white	I	Greenish white	I	5.92	6.00	A3, A3
ζ Lyrae	Greenish [Yellow]	I	White [Greenish]	I	4.29	5.87	A3, A3
19 Lyncis	White	I	White	I	5.61	6.53	
γ Leonis	Gold	II	Greenish red ?	II	2.61	3.80	Both solar stars

DOUBLES WITH *Comites* OF EARLIER SPECTRAL TYPES

Star	Color A	Spectrum A	Color B	Spectrum B	Mag. A	Mag. B	Remarks
β Cygni	Yellow	II	Blue	I	3.0	5.3	Early solar, late I type
γ Andromedae	Yellow	II	Blue	I	2.3	5.0	Late solar, late I
12 Canum Venaticorum	Yellowish	II	Bluish	I	3.2	5.7	Early solar, [early I
ι Cancri	Yellow	II	Blue	I	4.2	6.6	
o′ Cygni	Yellow	II	Blue	I	4.0	5.0	
δ Cephei	Very yellow	II	Blue	I	4.2	5.3	Late solar, I
α Scorpii	Fiery orange	III	Blue green	I–II	1.2	7.	
ε Boötis	Very yellow	II	Very blue	I	2.7	5.1	Late II like *Arcturus, comes* late I. K, A
32 Eridani	Yellow	II	Blue	I	4.9	6.3	Late II like *Capella, comes* I. G5, B ?
α Herculis	Very yellow	III	Very blue	I	3.0	6.1	*Comes* early I

DOUBLES WITH *Comites* OF LATER SPECTRAL TYPES

Star	Color A	Spectrum A	Color B	Spectrum B	Mag. A	Mag. B	Remarks
59 Serpentis	Yellow	I	Blue	II	5.5	7.8	A, G
ξ Cephei	Yellow	I	Blue	II	4.6	6.5	A3, G
	White		Tawny or ruddy				
	White		Yellowish				Sestini 1844
μ′ Boötis	Flushed white	I–II	Greenish white	II	4.5	6.5	F, K
	Yellow		Yellowish azure				Sestini 1844
30 Arietis	Yellowish	I–II	Bluish cast	II	6.1	7.1	F, F5
95 Herculis	Greenish	I	Yellowish	II	5.1	5.2	A3, G5, or K

primary, substantially always of type I. The primaries are all of type II or type III, and the differences in magnitude are generally much greater than in the list just considered. There is in all the stars of this list an undoubted slight difference in color between the primary and the *comes*, but it is not at all such a difference as is indicated in the colors given. For example, in the first star of the list, *β Cygni*, the apparent colors are notoriously very yellow and very blue. On the other hand, it is questionable whether any star of the earlier solar type can reasonably be classified as very yellow, and it is entirely certain that no star of the later first type can, save by a prodigious stretch of the imagination, have attributed to it the blue color of the *comes* of *β Cygni*. Franks[1] notes, and very properly, that some stars of the same visual color are of different spectral types, and, conversely, some stars of the same spectral type may differ perceptibly in color, but neither he nor any other experienced observer has detected an isolated late first-type star of the color of this *comes*, or has made any provision in the tabulation of star colors for any such extraordinary phenomenon. In fact, in an earlier paper Franks[2] himself notes *β Capricorni* as having a *comes* which is probably white, being of the first type, and merely blue by contrast. The same general considerations hold for the rest of this list. Finally, there is a brief list containing five doubles with *comites* of later spectral types than the primaries. These have primaries of type I and *comites* of type II. The first on the list, *59 Serpentis*, is typical. It is a distinctly yellow and blue combination in which the yellow star is of type I. Now a type I star may well enough be yellow, but certainly a type II star, bright blue in color, is something for which one may search the heavens in vain. The difference in magnitude between the two components is here over two magnitudes. The other stars of the list have somewhat less striking coloration, and the epithets applied to the *comites* will be shown later to have much significance. The last star of the list is a famous apparent color variable, the vagaries of which are worth separate discussion. In brief it appears from Table I that a double having a primary of type II or type III with *comes* of type I will uniformly

[1] *Loc. cit.*

[2] *Ibid.*, **67**, 539, 1907.

show a contrasted coloration of its *comes*, quite distinct from anything possibly justified by the spectral type. Even β *Cygni*, which, when investigated visually many years ago by Sir William Huggins, appeared to show possible cause for a considerable color difference, displayed, when the same distinguished observer actually got the spectrograms on a large scale, characteristics that indicate a mildly yellow primary and a probably whitish *comes*. Inasmuch as the stars of Table I are all the doubles for which the complete data are available from the Harvard publications, they may be fairly considered typical of the conditions represented.

It is not putting the case too strongly to state that all the colors observed in double stars, and not pertaining to stars in general, are purely of subjective origin and due to causes readily assignable from the data of physiological optics.

The effect of contrast in double star colors has often been recognized, but it has been very generally dismissed from serious consideration as the actual cause of all such colors, by reason of two arguments. First, that the striking colors of many of the *comites* must be real because they persist when the primary is hidden by an occulting bar, and second, the argument advanced by Struve that if such colors were contrast colors they must be complementary, which they are not accurately, save in a comparatively small number of instances. Both arguments are in view of present knowledge of the theory of vision utterly fallacious. In the first place, colors due to subjective causes, which will presently be assigned to them, should not and do not disappear by the use of the occulting bar as ordinarily employed, and second, the observed colors, considering the physiological causes of their origin, can be accurately complementary only in especial cases and generally must vary materially from such character. The fallacy in the two stock arguments is due in the main to an entirely unwarrantable assumption that apparent color variations in such cases should be charged to simultaneous contrast.

In point of fact the subjective colorations observed are chargeable to four separate causes, each of them now well known and among which simultaneous contrast is only of minor importance. Simultaneous contrast, which involves the shifting of contrast colors toward complementary effect, is quite probably mainly a psychological

rather than a physiological phenomenon, inasmuch as it may occur to a certain extent in very brief glimpses (during which there is reason to expect it), as shown by Mayer, and once introduced sometimes persists, as Helmholtz has noted, until the eye gets rid of its bias and takes a fresh start. A far more important source of subjective coloration is fatigue color, whereby fatigue of a color sensation dulls that particular hue and brings out with relative intensity its complementary.

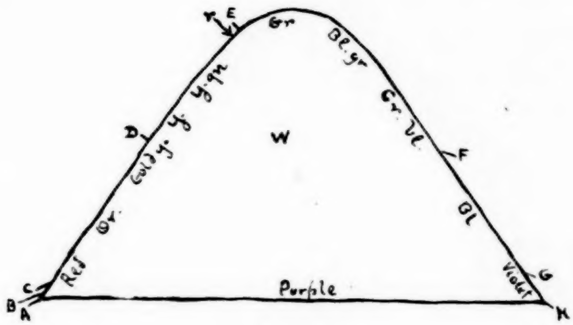

FIG. 2

Fig. 2 shows Maxwell's color diagram as given by Fick, and displays the ordinary range of complementary colors seen in simultaneous contrast and fatigue. A line passed through *W* from the stimulus color shows at its intersection with the boundary beyond *W* the complementary tint. Experiments on complementary colors in this ordinary sense are familiar and need not here be inserted, but both in the case of simultaneous contrast and of fatigue color the subjective hue conferred by the stimulus color is only truly complementary in the sense of this color diagram when the ordinary hue is unmixed with any coloration of the background, subjective or objective. Hence in applying such a color diagram to the case of double stars one would find the true complementary produced only in the absence of any subjective or objective color in the *comes*. The primary would excite, it is true, its complementary tint, but the blending of this with faint actual coloration of the *comes*, or with spurious colorations now to be considered, will throw the visible tint in many, and probably a large majority of cases, wide of that rigorously complementary to the pri-

mary. A third cause operating to produce subjective color is the shifting of the retinal color sensitiveness away from the red in faint illumination, following Purkinje's phenomenon. The facts regarding this are now well known and are shown in Fig. 3, reduced to the normal spectrum from the researches of Sir William Abney.[1] Here curve *A* shows the relative luminosities from the various parts of the spectrum for the normal eye and ordinary intensities of illumination, say a few meter-candles. Curve *B* shows the luminosity value

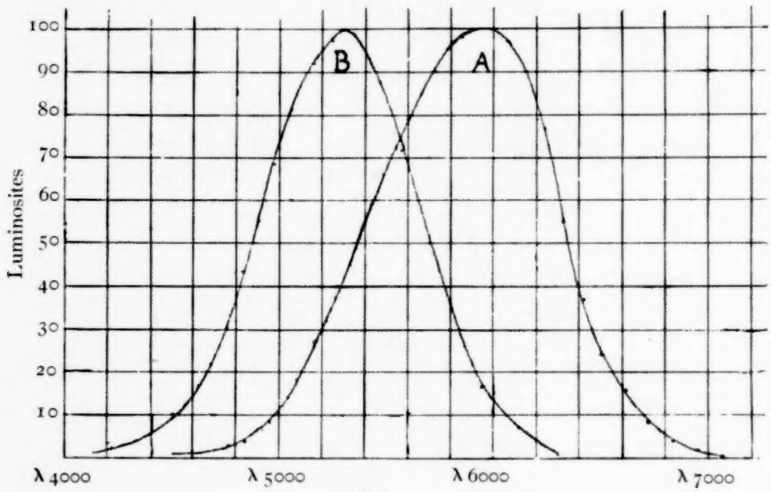

FIG. 3
0.0066 + *c* = 0.06 meter-candle

for the normal eye at very low intensity of illumination, about one-twentieth of a meter-candle or thereabouts. The maximum of luminosity has here been shifted from near the D lines almost to E. A white object at such intensity of illumination therefore has, as will be readily seen from inspection of the curve, a predominant greenish-blue hue, and this hue may be anything between the limits set by curve *A* and those set by curve *B*, according to the intensity of the stimulus. At a still further reduction of intensity to near the vanishing point even the bluish-green tinge fades, when the eye is in its ordinary stages of adaptation, into an indeterminate sort of whitish. The point to which the maximum luminosity shifts in curve *B*,

[1] *Colour Vision*, p. 103.

which corresponds to what is now well recognized as "rod vision" as distinguished from "cone vision," is marked *r* in the color diagram of Fig. 2. It becomes obvious, therefore, that in observing any very faint objects, irrespective of their real colors, the tendency is to see them of faded blue green merging into a whitish hue, or at least to rob them in very large measure of red and yellow tints, the latter condition marking the transition stage between cone and rod vision. This fact is sufficient cause for the progressive blanching of faint stars and also for the bluish tints which Smyth attributed to some of them. Inasmuch as from Pickering's comparisons[1] a star even as bright as the fifth magnitude would be matched by a candle at a distance of about 5.26 km, it is obvious that even the light-gathering power of comparatively large telescopes leaves relatively small illumination to serve as stimulus, and in the observation of stars like faint doubles one is continuously working near the limits of vision, a fact which is especially significant when one realizes that most of the color observations upon doubles have actually been made with small instruments, say from 12 to 16 cm in aperture. A fatigue color impressed by a primary on a considerably fainter *comes* of which the color has been thus much modified cannot be expected to be, and is not, accurately complementary. Finally, as a powerful and very interesting modifying influence in observed colors one must reckon with the "dazzle tints," that is, the subjective colorations corresponding to positive after-images. These, which may and do occur even in faint illumination when the eye is properly adapted, may produce very extraordinary modifications of color and are chargeable with some of the most remarkable phenomena noted in double star observations. The whole subject of fatigue colors and dazzle tints is set forth in a masterly paper by Burch.[2] The sequence of dazzle tints runs through reddish hues to green and then to blue and violet. Such tints are disclosed after a brief glance at the sun, but the sequence as stated holds for very weak illuminations, as Burch showed in his later paper,[3] and terminates with violet, while abolition of the reddish dazzle tint under such circumstances takes of itself a longer time than

[1] *Harvard Annals*, **61**, 69, 1908.

[2] *Philosophical Transactions*, B, **191**, 1, 1900.

[3] *Proceedings of the Royal Society*, B, **76**, 199, 1905.

is generally allowed for resting the eye in any ordinary observations and the sequence finally and definitely terminates with the fading of the violet subjective hue after a period approaching two hours. Under extreme conditions Burch found extraordinary phases of temporary color-blindness produced by dazzling, so that, for instance, the *b* lines of the solar spectrum appeared in a field of bright red, bright green, or bright blue, as the case might be. Tiring the retina by light from the vicinity of the D lines affected both the red and green sensations, the effect of the positive dazzle tint being to give a purplish cast to the violet. This red dazzle tint, by the way, is well seen in the positive after-image of a yellow "flaming arc." As the reddish dazzle tint died out it was succeeded by a greenish one and the red end of the spectrum began to reappear. Under some circumstances the fatigue effects produced apparent weakening in portions of the spectrum, such as would look in very faint light like absorption bands, showing the extreme caution which must be exercised in the visual observation of very weak spectra when stray light is not absolutely excluded from the eye and the eye itself long adapted by complete darkness.

With these four causes of subjective coloration in coincident operation it is evident that truly complementary hues are likely to be the exception; and the predominance of the quasi-permanent effects due to fatigue and to dazzle tints, effects which disappear only after a few or many minutes of complete relief from the primary stimulus, shows the improbability of any disappearance of such causes of subjective color in the ordinary use of the occulting bar or in the course of lunar occultations. These facts also make it evident that in observing objects like double stars, assuming the colors to be subjective, brilliant colors normally appear only when the difference in magnitude between the components is considerable, so that the primary can impose its fatigue and dazzle tints upon the faded light of the *comes*. This condition is exactly what is found in actual double star work, that is, in most cases the colored *comes* is very much fainter than its primary; and where the difference is relatively small, say only a fraction of a magnitude, the colors are peculiarly shifty and uncertain, varying both in intensity and in hue as will be noted later.

In order to test the efficiency of these physiological causes in producing the colors of double stars, the writer proceeded to the investigation of these phenomena by means of artificial stars. To this end the apparatus of Fig. 4 was constructed. It consisted of a light-tight blackened wooden box, a meter long and about 10 by 13 cm in cross-section, shown at *A* of the figure. Midway of the box and extending to half its height inside was the hard rubber diaphragm *s*. The farther end of the box carried a photographic plate-holder *P*, in the upper part of which the septum had been cut through in a space of about 2 by 3 cm. Diffused light was thrown upon this plate-holder by a white matt paper surface *R*, about 20 cm from the plate-holder, illuminated by a gas burner at *L*, which could be varied

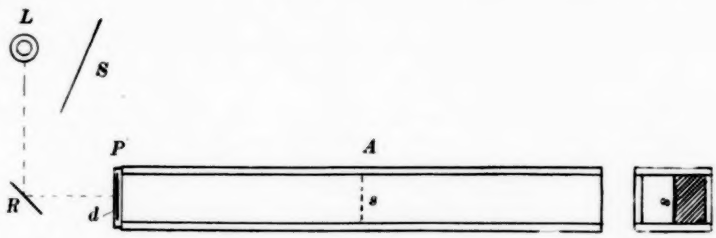

FIG. 4

in distance so as to make the complete distance to the plate-holder from 0.5 m to 1 m. The screen *S* cut off the light from the observer. In one of the slots of the plate-holder *P* were placed the necessary perforated diaphragms to produce the artificial stars, and across the upper half of the aperture in the septum there was provision for sliding a photographic wedge so as to cover one of the double star apertures in the diaphragm. Provision was also made for covering either or both of the apertures by tinted glasses of the same or different colors. The color of the light itself could also be modified by using a tinted diffuser at *R*. The purpose of the diaphragm *s* was to enable one of the stars to be occulted merely by dropping the eye a trifle in observing it. The apertures employed were holes through the hard rubber slide of the plate-holder of various sizes placed in various relations. In the preliminary experiments four sets of apertures were used: *A* and *B* 3.25 mm and 2.1 mm in diameter, respectively; apertures *C* and *D* each 3.25 mm in diameter; apertures *E* and *F*

each 1.6 mm in diameter, and apertures *G* and *H* each 0.8 mm. The uniform distance between the centers of these artificial stars was 9.8 mm, corresponding in angular separation therefore to a double star with a separation of 10″, as observed under a power of about 200. The writer should here state that he is possessed of absolutely normal color vision so far as pretty thorough testing in spectroscopic and other work has disclosed, and of about a normal degree of acuity. The photographic wedge used for cutting down the light of one of the components was made at the Harvard Observatory and is of the type regularly used in stellar photometric work there and elsewhere. These photographic wedges are well known from the investigations at the observatory to be free from selective color absorption. Two such wedges were used in these experiments, one ranging up to 18 stellar magnitudes of difference, that is, to practically complete opacity, and the other on a wider scale ranging up to 7 magnitudes. As a check upon these a double-image prism, and a thick block of Iceland spar producing the same effect, were used in conjunction with a Nicol prism as analyzer for cutting down the light of one of the components. The same color phenomena were noted with the polarizing devices as with the wedge, and the latter were used on account of their greater convenience and especially on account of the facility with which doubles differing in apparent size as well as intensity could be produced. Observations were conducted in a darkened room, lighted only by stray light from the working burner. A small slitless spectroscope by Hilger, used with or without a weak cylindrical eye lens, enabled the apparent spectra of the artificial doubles to be investigated. The prisms of this instrument were those customarily supplied by Hilger for small ocular spectroscopes for stellar work.

To summarize briefly the results of these experiments, they showed that merely cutting down the intensity of one of the elements of the artificial double invariably shifted its color progressively from the initial color through a variety of transition hues, sometimes complicated by the appropriate dazzle tints, to a distinctly greenish, bluish-green, or bluish color varying somewhat with the intensity of the illumination and with the whitish or yellowish casts given the stars, and sometimes reaching the glittering contrast found in doubles

like β *Cygni* and ϵ *Bootis*. When the *comes* is reduced to bare visibility, practically all color may be lost from complete failure of color sensation. Moreover, these subjective colors in the *comites* do not disappear on occulting the primaries, unless after a protracted rest of the eye in darkness. When such rest was taken, the colors were dim only for a brief period and came back quickly from simultaneous contrast and increasing fatigue.

Shifting the observation from the tired eye to the fresh eye changed the hue somewhat but did not abolish the subjective colors, which, perhaps from having become firmly impressed on the mind, were the more readily picked up by simultaneous contrast. On starting a set of observations with an absolutely fresh eye rested by darkness, the colors observed were usually weak, but by the time the eye had fairly settled down to close observation they were rapidly enhanced toward the usual results. A little exposure to light before beginning brought on the tints more quickly and sometimes they appeared almost at the first glance even when the eye was in the condition of rest, particularly if there was a slight initial color difference between the elements of the double, such as might exist between stars of type I and type II. Table III, culled from the original observations, gives a good idea of the normal effects.

In the case of the first set of readings it will be observed that the artificial stars were both weak in the blue on account of the yellowish source of light, and the colors were less conspicuous than in the later sets. In particular there were observed curious flickering effects of the smaller yellowish star, with an uncertain blue-green tint washed over it. This effect is common during the transition tints prior to the final contrast at the end of the experiment, and it was very noticeable here as always that occulting the primary produced no change in the color of the *comes* unless the eye was rested for some minutes. After this the length of time required to bring the *comes* to a different tint varied considerably, but it never came clear back to the color of the primary, since when very weak it would necessarily appear somewhat blue green on account of the shift in luminosity in weak light. Occulting the primary in such cases merely slowly weakens the fatigue and dazzle tints and eliminates whatever effect may be obtained from simultaneous contrast. With the spectroscope

TABLE III

NOVEMBER 14, 1909. SOURCE, GAS JET GIVING A CLEAR LIGHT YELLOW
APERTURES *E* AND *F*

Primary	Color	Δm	*Comes*	Color
E	Yellowish	1	F	Same a little dulled
E	Yellowish	2	F	Much paler tint
E	Yellowish	3	F	Yellowish with distinct greenish cast
E	Yellowish	4	F	Stronger cast, flickering and dubious
E	Yellowish	5	F	Distinct blue-green cast, shifty

Colors do not change in *comes* by occulting primary.

DECEMBER 5, 1909. SOURCE, FRESH WELSBACH MANTLE, WHITISH
APERTURES *E* AND *F*

E	Yellowish white	1	F	Paler and faintly greenish white
E	Yellowish white	2	F	Faintly bluish green
E	Yellowish white	3	F	Contrast same but stronger
E	Yellowish white	4	F	Bluish green, contrast striking
E	Yellowish white	5	F	Rather vivid blue green

E paled a little from fatigue toward the end and contrast then came out strongly with the other less fatigued eye. Colors remained on occultation.

DECEMBER 1, 1909. WELSBACH WITH LIGHT AMBER GLASS OVER PRIMARY
APERTURES *A* AND *B*

A	Yellowish	1	B	Slightly pale in hue
A	Yellowish	2	B	Distinctly whiter than A
A	Yellowish	3	B	Faint bluish cast evident
A	Yellowish	4	B	Stronger bluish, A looks yellower
A	Yellowish	5	B	Still stronger, decidedly blue
A	Yellowish	6	B	Contrast strong as in β *Cygni*

Color of *comes* has an odd flickering quality, now caerulean now merely dull violet, sometimes almost purplish. No change on occultation of A.

DECEMBER 15, 1909. WELSBACH WITH LIGHT BLUE GLASS, GOOD WHITE
APERTURES *E* AND *F*, *G* AND *H*

E	White	1	F	*Comes* has faint tinge of bluish
E	White	2	F	Same a little stronger
E	White	3	F	Almost violet blue
E	White	4	F	Stronger blue but duller
E	White	5	F	Very strong bluish
G	White	1	H	Faded greenish blue, washed effect, shifty color
G	White	3	H	Greenish blue, very distinct
G	Greenish white ?	5	H	Vivid blue spark

None of these colors changed by occulting primary.

the spectrum of the *comes* was very faint in the red end from weakening of the red sensation.

The second set of readings with a fresh Welsbach mantle as a source of light gave decidedly stronger effects, as might be expected. The subjective colors came on at a less difference of magnitude and were altogether more striking. The fact of fatigue was plainly shown in the primary by apparent paling in color. In the third set of readings the primary star was given a strong yellowish tint by a colored glass screen, and here the contrast in colors became ultimately very striking, quite as brilliant as in the case of β *Cygni*. A bright yellow primary easily turns a whiter *comes* blue, whether with real or artificial stars. During the transition period when the colors were beginning to appear, the usual curious uncertain quality of the color was very conspicuous and especially casts of violet and purplish, the necessary result of the dazzle tint of the brighter star. In the fourth set of readings both stars were screened with a very light blue glass, bringing up the color of the Welsbach to a good white. The effects were very brilliant and during the transition period there was the usual tendency to shifting color. These observations of Table III are typical of many, all leading to the same general results, which have been checked by five observers besides the writer. Color difference as a rule sets in at about $\Delta m = 1$, and increases progressively. As between one observer and another, differences in the names of colors may be noted, while the phenomena described are obviously the same. For example, one observer will call a dull yellow in the beginning of a series of observations fawn color and another brownish, or later in the series the *comes* may be called bluish or greenish, while the dazzle tints also come in with varying effect, so that lilac, violet, and purplish are about equally likely to be reported. But a series always begins at small difference of magnitude with a slightly varying tint of the primary color at the start when the eye is fresh, passes through at medium differences of magnitude the various transition tints, and progresses to green, blue, or sometimes violet of a rather striking character. Some of the most curious effects are obtained when a dazzle tint is superimposed on the dulled tint of the *comes*, producing shades of ruddy brown, purplish grey, violet grey, and lilac. These colors are extremely suggestive of

Struve's remarkable epithet, "olivacea subrubicunda," applied to the *comes* of *ξ Orionis*, which has been variously described by others as light purple and azure.

In fact the whole curious list of colors given at the beginning of this paper may be picked up in these observations on artificial stars before the difference in magnitude has become great enough and the eye sufficiently settled into its fatigued state to produce the final color contrast. A very little experience in observing these artificial doubles makes comprehensible the differences in color assigned to certain stars by various observers, the color seen depending merely on the condition of the retina as regards the effect of fatigue and dazzle tints, and the difference in magnitude between the components. For example, *γ Ceti*, with a difference of magnitude of 3.8 between primary and *comes*, has uniformly had its primary classified as yellowish, while the *comes* has been called "ashy," "tawny," "olive green," and "ruddy or dusky," by various observers. "Ashy" is a term confined in double star observations mostly to cases where there is considerable difference in magnitude and the *comes* is relatively very faint and apparently of a dull washed-out bluish color. "Tawny," "olive green," and "ruddy or dusky" are tints entirely typical of the varying superposition of the fatigue and dazzle tints which one readily gets with the artificial stars. The exact effect of these cannot be predicted, but it is always of the general type here described. All observers seem to get similar results, but in very varying degree according to the condition of the eye at the beginning of the experiments and its sensitiveness to slight color changes. The color contrasts being in this way a function of the condition of the eye, it is not to be wondered at that various observers of double stars have reported a great variety of tints, the exact thing seen in any given observation being dependent on what the eye has been doing previously; for instance on whether the observer has been reading his position circles by too bright a light or has been observing some brilliant pair just before shifting to a fainter one. A careful study of *Antares* and its companion would certainly show effect in other observations for some minutes.

Following all these observations on artificial doubles the writer extended his observations next to artificial triples with rather striking

results. To this end a slide was given a combination of three apertures, *A*, *B*, and *E*, of the previous list, *B* and *E* being about 1 cm above *A* and about 7 mm apart, so that when a photographic wedge covered these two apertures the difference in magnitude produced was about 1.1. Thus appeared a primary star below, and two *comites* differing in magnitude above. Observation of this artificial triple star gave some rather beautiful color effects, the *E* aperture farthest along on the photographic wedge taking on its full bluish cast, while the *B* aperture was still in a transition stage and showing a variable wash of color. For instance, when the primary was a good yellow white, *B*, two magnitudes fainter, would show the merest trace of a blue wash, while *E*, 1.1 magnitudes fainter still, had come to a bluish washed with purple. The exact hue observed varied from experiment to experiment, but always ended with *E* a strong blue or blue violet, while *B* was weakly bluish, or bluish green, or faint purplish. The effect varied a little as between observer and observer, but the general results remained the same, and occulting the primary changed neither of the others in color until the eye had had a liberal time in which to recover. Another set of apertures differing slightly in arrangement and size gave entirely concordant results.

Finally an experiment was devised to see the possible effect of subjective coloration in the observation of star clusters. For example, Sir John Herschel described the loose cluster κ *Crucis* as like a gorgeous piece of jewelry in the various colorations of its components, blue, green, and reddish tints being most conspicuous. If one must consider the colors in double and triple stars as almost wholly subjective and involving in reality only such small color differences as exist between first-type and the second- and third-type stars, then there is a strong inherent probability that colors thus reported in clusters are also largely subjective, and in fact subsequent observers have been seldom able to see them as Herschel described them. An artificial cluster was therefore constructed consisting of 16 apertures of various sizes in a space of a little less then 2 cm square. The apertures were merely pin holes of various sizes. They were covered to reduce the light by a bit of thin white tissue paper and three large and centrally located apertures were then carried through the tissue

paper. The cluster thus presented three bright stars and 13 fainter ones. The result when this combination was put in the plate-holder of the star-box was absolutely startling; the large apertures were yellow white while the smaller ones varied greatly in color, running from dull yellow through greenish, violet, and purplish tinges according to the condition of their illumination and the state of the retina. A second similar cluster containing 19 apertures varying from 0.15 to 0.75 mm in diameter was then constructed, the apertures being covered with white tissue paper as before, with three of the largest apertures continued clear through the tissue. Over one of these large apertures was placed a shred of orange tissue paper to give a genuine initial tinge to one of the stars in the cluster. With this addition the effects were even more brilliant than before, the single orange star acting as a sort of *agent provocateur* to start the rest of the group into parti-colored activity. Viewed in the star-box the two brightest apertures were a good yellowish white, a third, the fine orange given by the tissue paper, and the others varied according to conditions through yellowish, purplish, bluish, and greenish hues, the small stars being as a rule conspicuously blue or green and with weakened light growing almost colorless. This result was checked by three observers besides the writer and while no two would have described the individual stars in exactly the same terms, save for the three large ones, each saw substantially the same thing, that is, the small stars were of some shade of blue or green, and the medium-sized stars yellowish occasionally running to purple or ruddy tints. From these experiments it appears that not only in the case of doubles, but in the case of multiple stars and in clusters, subjective colors play the chief, if not the only, rôle, in determining the apparent tints observed. The colors seen in double and multiple artificial stars of known equality or approximate equality of colors are quite sufficient in degree and in kind to account for the double star colors which have been reported in astronomical literature. Some initial color differences there certainly are, as the spectra show, but the actual colors do not vary enough to account in any material degree for the strange hues which have been reported. The subjective colors arising from the causes here set forth are, however, fully adequate to account not only for the extremely great differences in color reported, but

for the curious and evanescent tints which have so put to the test the descriptive powers of those who have noted them. The rôle of the dazzle tints is particularly noteworthy as bearing on the roseate, lilac, and purplish hues never observed in isolated stars and very far from affording complementary tints to their primaries. An extension of these results to the remarkable cases of apparent variations in the colors of double stars is now under way and will be reported later. The writer's particular thanks are due to Professor E. C. Pickering for friendly interest in this investigation and for the resources he has kindly placed at the writer's disposal.

BOSTON
February 3, 1910

ON CERTAIN STATISTICAL DATA WHICH MAY BE VALUABLE IN THE CLASSIFICATION OF THE STARS IN THE ORDER OF THEIR EVOLUTION[1]

By J. C. KAPTEYN[2]

Frost and Adams have brought to light the very significant fact that the helium stars have exceptionally low peculiar[3] linear velocities. This property of the bodies which are generally considered as representing the earliest stage in stellar evolution seems to be particularly promising as an aid in fixing the successive stages in the life-history of the stars.

For it seems to indicate that, as a rule, the velocity of the star's motion increases with age and, this being so, we conclude that the helium stars must owe their origin to heavenly bodies having still lower velocities. Here then we have at once a test for the theory which assumes that the stars are evolved from nebulae. We know little about the velocities of the nebulae; but, as will be seen below, what we do know concerning them points to a very considerable linear motion instead of a vanishing one.

Have we to conclude that the nebulae are *not* the parents of the stars? With one exception the nebulae whose radial velocities have been measured belong to the class of planetary nebulae. For the one exception, the *Orion* nebula, the peculiar velocity is vanishing. We should conclude, therefore, that the planetary nebulae do not stand at the beginning of the series, but rather at the end. The fact that the changes in the light of the new stars finally make their spectra identical with those of the planetary nebulae seems not unfavorable to such a conclusion.

As will be seen below, the radial velocity of these objects exceeds that of the helium stars to such an extent that, even notwithstanding the very restricted number of objects measured, our conclusion seems

[1] *Contributions from the Mount Wilson Solar Observatory*, No. 45.

[2] Research Associate of the Mount Wilson Solar Observatory.

[3] In accordance with custom we call *peculiar* velocity the velocity freed from that part which is due to the motion of the solar system through space.

already pretty well established. We cannot invoke, for the explanation of the displacement of the spectral lines, causes other than the Doppler effect, because the magnitude and the sign of these displacements are altogether different for the different objects.

On the other hand, the irregular nebulae *may* prove to be the birthplace of the stars. The case of the *Orion* nebula is decidedly favorable to this view, which harmonizes well with what we know from independent evidence. Still, it is clear how important must be the attempt to collect sufficient data for this and for other classes of nebulae—the white nebulae, the spirals, and the nebulous stars.

The importance of radial velocities for purposes of classification must not be restricted to the beginning of the series. Using all the data that I found it possible to collect, I obtained the following table of average values for the radial peculiar velocities, these velocities being all taken with the positive sign. The main value of the table lies in the fact that the number of *Orion* stars is relatively so great. This is owing to the courtesy of Professor Frost, who kindly placed the results obtained at the Yerkes Observatory at my disposal, so far as they rest on two or, with a few exceptions, on three or more plates.

Stars whose velocities were measured or published only because the astronomical or radial proper motion was exceptionally large were excluded. Such cases must vitiate the results. Spectroscopic doubles for which the velocity of the center of mass is well determined were included. The correction for the sun's motion was made on the assumption that the co-ordinates of the apex are

$$\alpha = 269^\circ.7, \qquad \delta = +30^\circ.8 \ (1875.0)$$

and the linear velocity of the sun 20.0 km per second.[1] The spectra are given in the well-known Harvard notation.

If we combine the results of all the second-type stars (F to K5), we find a regular increase in the peculiar radial velocity when we pass from the *Orion* (B) to the Sirian (A) stars, from these to the second-type stars (F to K), and, finally, to those of the third type (M); that is, if we follow what, according to our present knowledge, must be

[1] For the *Orion* stars the velocity given is computed with the sun's velocity equal to 23.7 km, a value derived from these stars themselves. With the sun's velocity equal to 20 km we should have found 6.9 km for the average radial velocity.

considered as the most probable succession of the different classes. The difference between the Sirian and the second-type stars is small. We have, however, further evidence that it must be real. It is true that Campbell found rather the reverse from his 280 stars.[1] But his classification is avowedly a very crude one. In fact he did not use any direct data relating to the spectra, but arranged his stars according to the amount of the difference, *visual magnitude – photographic magnitude*. These results need not be given any considerable weight, therefore, in the present instance.

Spectrum or Object	Peculiar Radial Velocity	Number
B to B9	6.5 km	64
A to A5	12.6 (11.2)	18
F to F8	14.5 } 14.5	17
G to G5	12.6 } 14.5	26
K to K5	15.4 } 14.5	55
Ma	19.3	6
Planetary nebula	26.8	13
Orion nebula	0.1	1
N	13.1	8
L	3.7	2
Total		210

On the other hand, we have: First, all the stars, not spectroscopic binaries, the velocities of which were measured or published only because the motion is exceptionally large, belong to the classes G and K—not a single one either to class B or to class A. The total number of these is only nine, but I think that later observations have corroborated the fact. Second, we have pretty strong evidence furnished by the astronomical proper motions. From these I have derived[2] the linear velocities projected on a plane, with the following results:

Stars of first type and unclassified stars	1.429 h
Stars of second type	1.494 h

where h is the linear velocity of the solar system. The difference, though in the expected direction, is small. At a later period, however, I separated the stars with unknown spectra from the rest and at the

[1] *Astrophysical Journal*, **13**, 84, 1901.

[2] *Astronomische Nachrichten*, **146**, 97, 1898.

same time followed a somewhat safer method in forming averages. I then found:

Type II (F to K)	1.46 *h*	(1093 stars)
Type I (B, A)	1.02 *h*	(1144 stars)
Type unknown	1.45 *h*	(381 stars)

A considerable number of the nonclassified stars are probably of the first type. It is likely, therefore, that if we knew the spectra of these stars the difference between the first two values would be diminished somewhat. We have to consider further that among the type I stars have been included a few helium stars. For the A stars separately we would thus have found a somewhat greater value. As, however, the number of B stars is only one-eighteenth part of that of the A stars, the change must be small. On the other hand, the influence of the errors of observation, which are much greater for the A stars owing to the smallness of their proper motions, must have been to diminish the difference between the two types.

In conclusion, I think that the evidence of the astronomical proper motions leads us to the belief that the ratio

$$\frac{\text{average linear velocity of the F, G, K stars}}{\text{average linear velocity of the A stars}}$$

cannot be smaller than 1.3. If we accept this value from the radial velocity 14.5 km for the stars of the second type (see table), which must be fairly well determined, we derive for the Sirian stars a velocity of 11.2 km. Thus there seems to be good reason for admitting that the velocities increase regularly if we pass from the B stars to the A stars, and from these to the stars of the second type. Does the velocity still increase when we pass from the second-type stars to those of the third? The six stars of this latter type for which I have been able to find radial velocities give indeed the considerably greater value 19.3 km. But of course the number of stars is too small to allow of any safe conclusion.

The above table is both incomplete and provisional. The time cannot be far distant, however, when we shall have the means of establishing it on an adequate number of observations. When this has become the case may we not hope that such questions as the following may be settled, or at least be brought much nearer to their

solution? Do the fourth-type stars (N) represent a later or an earlier stage than those of the third? Do the Wolf-Rayet stars represent an early or a late stage? Do they stand at all near the planetary nebulae, as Pickering thinks, or are they more nearly related to the helium stars? Are the *novae* in their last stage really to be considered as ordinary planetary nebulae? What is the place of the helium stars showing bright lines? And such more general questions, as the following: Is the order of evolution, O, M, (G), A, (G), N,[1] as proposed by Lockyer, tenable?

The phenomenon of the increase of velocity with the evolutional stage of the stars must give rise to speculation as to its cause. The observational results contained in our table naturally lead us to conclude that the matter from which the stars originate must have little or no velocity. How is this possible under the influence of the combined attraction of the rest of the system? Is it not *as if*[2] gravitation had no effect on the cosmical matter in its primordial state? If this be so, then no relative motions of its several parts will arise: they will remain at rest relatively to their common center of gravity, that is, relative to the same center to which the velocities in our table are referred. As soon as matter changes from this state to another in which gravity begins to act, or to act freely, motion will arise; and it is evident that, as a rule, this motion must be accelerated, at least during immense periods, so that, the longer the period elapsed since the birth of the stars, the greater must be their average velocity, which is just what we find to be the case.

It may be well to direct attention to other phenomena by which

[1] Some of the G stars would be on the ascending, others on the descending branch of the curve. The data already available seem hardly reconcilable with Lockyer's series.

[2] We need not necessarily make the hypothesis that really primordial matter is not subject to gravitation. If, for instance, as was suggested to me by a friend, the tenuity of this matter were such that it were very materially hindered in its motion by the matter which we must assume as filling the universe in order to explain the phenomenon of selective absorption of light recently found, the velocity of this matter could not exceed the value for which the resistance is equal to the total attraction. We have only to assume that this is the case for a relatively low value of the velocity. Other suppositions may probably be made of forces which, in the primordial state of matter, counteract gravity. But it is evident that in such cases, where gravity is just counterbalanced by another force, things happen *as if* there were no force at all.

we are driven to a similar conclusion. In the *Hyades*, the *Pleiades*, *Ursa Major*, etc., we have physical groups of stars, the motions of which are parallel and equal in amount. It is certain that, under the influence of the mutual attraction of the members of the groups, and in part also under the influence of the attraction of other stars, this parallelism and equality cannot continue to exist indefinitely. The time must inevitably come when they will be so thoroughly destroyed that no appreciable trace of a community of motion will be left.

But if it be true that, under existing circumstances, this community of proper motion can exist only temporarily, how does it happen that it exists at all, that it was not destroyed long ago? The only answer to this question at all satisfactory that I can find is, in accordance with the conclusion to which we were led a moment ago, that there has been a time when the matter now composing such groups was in a state in which things happened *as if* gravitation had no effect on it. For a group such as that of the *Hyades*, for which the parallax is now known with fair accuracy, and in which, according to a private communication[1] from Professor Frost, the number of spectroscopic binaries is particularly great, so that data concerning the matter will soon be available, we shall be able to determine roughly the time necessary to produce internal motions in the group of an amount equal to that which the observations allow us to assume as possibly now existing. This interval will be the maximum interval during which the system can have existed abandoned to the normal and unchecked action of mutual gravitation.[2]

Again, the same question is presented to us, on a larger scale, by the stellar system as a whole. The members of the two star-streams, which seem largely to constitute the system, must, by their mutual attraction, destroy in the long run every trace of the now existing regularity of the motions. That the stream motion is still recognizable at the present time must be due to the fact that the perturbing forces have not effectually worked for an indefinite time. The star-

[1] See also *Astrophysical Journal*, **29**, 237, 1909, and **31**, 178, 1910.

[2] I have already made, some time since, an estimate based on what seemed to me a plausible hypothesis as to the matter. Now that we have the prospect of getting better data soon, I prefer to suppress my provisional results.

streams, too, therefore point to a time when gravitation apparently or really had no effect. Another phenomenon might finally be mentioned in this connection, to which several astronomers have already called attention, namely, the fact that the Milky Way has not long since been dispersed. But I think that enough has already been said to justify our point of view, and I prefer to devote the rest of this paper to the consideration of elements other than the linear velocities of the stars, which may render service in finding the position of certain classes of stars in the order of evolution.

It seems probable that the average *absolute* magnitudes are different for stars of different spectral classes, and that they will fall into a smooth curve when the stars are arranged in the proper order. Attempts have already been made to derive such average absolute magnitudes. I think, however, that such determinations must be illusory, at least as long as we have to deal with classes of stars, part of which our observations cannot reach. The intrinsically faint stars of every class must, as a rule, be found among the stars of faint *apparent* magnitude, and the spectra of these have not yet been classified. It seems that there is no hope of a really good determination of the average absolute magnitude of any spectral class before we can include stars of every class of apparent magnitude that furnishes specimens of the spectral class in question. There is, besides, the difficulty of the parallaxes; and the use of average parallaxes, which are more easily obtained, is highly objectionable for the present purpose. The outlook in this direction is therefore not promising.

As long as our data concerning radial velocities are so scanty, some help may be obtained from the astronomical proper motions.[1] In the main, however, the varying amount of these motions must be due to differences in distance, so that what they can teach us for the present question cannot be compared to what we may hope to learn, in the near future, from the radial motions.

There are other elements which seem more promising, though they need confirmation by further observations. In the first place: The observations at the Yerkes Observatory have brought to light an unexpectedly large number of spectroscopic binaries among the *Orion* stars. The percentage of the stars known to be spectroscopically

[1] *Astrophysical Journal*, **30**, 173, 1909.

double is far higher for this class than for any other type. Now it is difficult to imagine how binaries in their evolution from the earlier *Orion* stage to the later Sirian . . , solar . . types can, in great part, become single stars. It seems much more satisfactory to assume that the higher proportion of binaries among the *Orion* stars must be apparent and due to some circumstance which makes the discovery easier for this class of objects. Such a circumstance would be given should it appear that the periods are longer for the older types. For the longer the period, the longer the binary character of the star may escape notice. The data at present available indeed show such an increase in the period very clearly. From Campbell's list of spectroscopic binaries,[1] to which I have added 15 binaries whose orbits have subsequently been determined,[2] I find:

Period	Number of Stars			Percentages		
	B, A	F, G	K, M	B, A	F, G	K, M
0^d to 10^d	22	14	0	65	41	0
$>10^d$	12	10	4	35	59	100

For a great number of stars, for which the period cannot yet be determined accurately, we can still judge from the observations as to whether it will turn out to be long or short. According to Campbell's catalogue we thus have in addition to the above:

Period	Number of Stars			Percentages		
	B, A	F, G	K, M	B, A	F, G	K, M
Short	18	3	1	90	33	17
Long	2	6	5	10	67	83

We thus see, as a simple result of the observation of a very moderate number of stars, that there can be no reasonable doubt as to the reality of the phenomenon. On the other hand, we know that theory demands a lengthening of the period under the influence of tidal attraction. Thus, I think, we may confidently look for a confirmation of our conclusion by further observations and computations.

There is another fact which may very naturally be explained by the present phenomenon, viz., the fact that, so far as we know, all the *Algol* variables belong to the first type. For it is evident that the chances of an eclipse are enormously diminished as soon as the period, consequently the distance, of the components increases.

[1] *Lick Observatory Bulletin*, No. 79, 1905.

[2] Visual binaries have been excluded.

In the second place, we have the above-mentioned gradual dissipation of the star-streams. According to the views here put forward, the cause of obliteration of the streams must have been longer at work for the stars of the later types, so that, for the younger spectral types, the phenomenon of star-streaming must show itself in greater purity than for the later ones.

We have already observational evidence that this is so. In his second paper on the systematic motion of the stars[1] Dyson finds that the stars of type I "diverge less from the general drift of the streams than the other stars." From Dyson's numbers I find for the average deviation of the first-type stars only three-quarters of that for all stars together.[2] Not only this. Observation shows further that for the *Orion* stars the stream velocity is small. This is already implied by the fact that the total motions of the *Orion* stars are small. But as the point seems important I have derived, on the basis of Eddington's theory, the values of both the cloud velocity and the average internal velocity[3] as far as they are obtainable from the radial velocities alone. For the apex of the sun's motion were assumed the co-ordinates

$$\alpha = 269^{\circ}.7, \qquad \delta = +30^{\circ}.8 \quad (1875.0).$$

The supposition that the sun's velocity is 23.7 km per second leads to a value of the average radial motion ($\bar{\rho}$) which near the vertex is actually slightly smaller than that at a greater distance (see footnote). This is incompatible with the two star-stream theory. The difference, however, is far within the limits of the errors of observation, and in making the computation I took the two separate averages both equal to the total average ($\bar{\rho}=6.63$). I thus found for the *Orion* stars:

[1] *Proceedings of the Royal Society of Edinburgh*, **29**, Part IV, 390.

[2] Deviations exceeding 60° being excluded in both cases.

[3] As is well known, Eddington's theory of the star-streams assumes two star-clouds in each of which the velocities are distributed according to Maxwell's law. I here simply assume the number of stars in the two clouds to be the same: the velocity of the center of gravity of the two clouds in respect to the center of gravity of the whole of the two clouds together will then be equal and opposite. This direction will meet the celestial sphere in what I called the true vertices; the velocity itself may be called the ***cloud*** or ***stream velocity***, whereas the velocity of the individual stars with respect to the center of gravity of the cloud to which they belong may be called the ***internal velocity***.

Let ω = cloud velocity;

$Ae^{-h^2(t^2+u^2+v^2)}dtdudv$ = number of stars having components of internal velocity between t, u, v and $t+dt$, $u+du$, $v+dv$, respectively;

Sun's Velocity	Cloud Velocity	Average Internal Velocity
20.0 km	5.8 km	12.1 km
23.7 km	0.0 km	13.3 km

These numbers must be compared with those obtained from all the stars, without distinction as to spectral type. These I derived from the astronomical proper motions in Eddington's paper. I find, again assuming an equal number of stars in the two clouds, and the sun's velocity as 20.0 km:[1]

Cloud velocity = 25.6 km,
Average internal velocity = 31.2 km.

λ' = angular distance of star from vertex;

$\bar{\rho}$ = average radial velocity, freed from the sun's motion through space, all the velocities being counted positive.

Then I find

$$\bar{\rho}=\frac{2h}{\sqrt{\pi}}\,\omega\cos\lambda'\int_0^{\omega\cos\lambda'} e^{-h^2x^2}dx+\frac{1}{h\sqrt{\pi}}e^{-h^2\omega^2\cos^2\lambda'} \qquad (a)$$

If $\bar{\rho}$ is known by observation for two widely different values of λ', this equation will furnish the values of ω and h. From this we get the average internal velocity Ω by the formula (given by Eddington):

$$\Omega=\frac{2}{h\sqrt{\pi}} \qquad (b)$$

If from the *Orion* stars used for the table I exclude the two whose spectra are peculiar, I get:

$\cos\lambda'$	Average $\cos\lambda'$	Average $\cos^2\lambda'$	$\bar{\rho}_1$	$\bar{\rho}_2$	No. of Stars
0.00 to 0.71	0.367	0.186	6.20	6.76	31
0.71 to 1.00	0.895	0.808	7.40	6.50	31

where $\bar{\rho}_1$ is computed on the supposition that the sun's velocity is 20 km per second; $\bar{\rho}_2$ on the supposition that the velocity is 23.7 km, as found from the *Orion* stars themselves. From the values $\bar{\rho}_1$ and $\bar{\rho}_2$ the values of ω and Ω were then obtained by the formulae (a) and (b). I take occasion to state in passing that in my own theory (not yet published) the formula which takes the place of formula (a) is

$$\bar{\rho}=\frac{\pi}{2h^4}(2a-b\sin^2\lambda') \qquad (c)$$

which with the values derived for the constants h, a, b, and the sun's velocity as 20.0 km per second becomes:

$$\bar{\rho}=25.07-12.38\sin^2\lambda' \qquad (d)$$

It was with this formula that the theoretical velocities were computed in my paper in the *British Association Report* for 1905.

[1] In accordance with Eddington's data (*Monthly Notices*, **67**, 34–63, 1907) I took
Co-ordinates of point Q_1, $6^h\ 0^m, -14°$; drift velocity, 1.65;
Co-ordinates of point Q_2, $19^h\ 20^m, -58°$; drift velocity, 0.50;
from which I derive
Co-ordinates of apex of sun's motion, $17^h\ 43^m$, $+31°.5$;
Co-ordinates of true vertices, $6^h\ 11^m$, $+1°$, and $18^h\ 11^m$, $-1°$;
Sun's velocity $=0.795/h$, which if equal to 20.0 km gives $h=0.0361$;
Cloud velocity $=0.922/h=25.6$ km.

Though the uncertainty in the amount of the cloud motion for the *Orion* stars is considerable, there cannot be the slightest doubt but that both the cloud motion and the internal motion must be very small as compared with the corresponding quantities for the rest of the stars.

Apart from the advantages that we may derive from this result for the classification of the stars in the order of their evolution, it has, I think, a great importance in its bearing upon the question of the generation of the star-streams themselves. For it proves that the streaming motion, too, is not an initial motion, but one generated at an epoch which, for the stars of any one type, must be placed at a time relatively but little preceding the time when they passed through the *Orion*-type stage.

In a lecture delivered before the Holland Society of Science on May 19, 1906,[1] the following questions were put:

> Have we to imagine that originally space was traversed by two independent star-clouds, similar to those clouds which we know, on a smaller scale, in the meteoric streams; that these clouds have met at some time in the remote past and have penetrated each other; and finally that the divergence in the directions and velocities have been caused by mutual attractions, both between the members of one and the same cloud and by the members of the two streams on each other?
>
> Or have we to deal with clouds which were not originally independent, so that the observed motions are to be attributed simply to the original form and the distribution of the star-density in the nebula that we call the stellar system?

In the light of the present discussion I think that the first of these two suppositions will have to be given up, so that the second thus gains immensely in probability. Howsoever this be, a more thorough investigation of the star-streams, *separately for stars of different spectral class*, seems highly desirable and promising, and we have placed such an investigation on the working program of the Groningen Astronomical Laboratory.

Leaving aside the more or less theoretical considerations, we may sum up what has been said as follows:

For the classification of the stars in the order of their evolution

[1] *Archives Neerlandaises*, Series II, Tome XI, pp. liii and liv.

the following, to be determined separately for the stars of each of the spectral classes, may be of great help:

1. The average amount of the radial velocity.
2. The average period of the spectroscopic binaries.
3. The average amount of the divergence of the astronomical proper motions from the general stream motions.
4. The quantity of the stream motions.

GRONINGEN
January 1910

THE ABSORPTION OF STAR LIGHT CONSIDERED WITH RELATION TO THE GALAXY

By GEORGE C. COMSTOCK

It appears to be well established at the present time that the major part of the lucid and brighter telescopic stars constitute two great groups, interpenetrating and flowing one through the other, and there is evidence indicating that the space occupied by these star groups is in some measure permeated by sparsely diffused meteoric matter, whose individual particles are of very small size and mass, but whose combined effect is to render less transparent the region that they occupy. If such be the case the motion of the two star groups must from time to time bring particles of the meteoric matter within the sphere of attraction of some particular star and impart to each such particle motion in a conic section having the star at its focus. The orbits thus formed must be predominantly hyperbolas and if either star group be conceived as having finite dimensions, the motion thus imparted to the meteoric particle must ultimately carry it outside this group without appreciably disturbing the arrangement of the group. There is thus a continuing tendency to sweep clear the space along the line of motion of the star groups and to produce in this direction a region of maximum transparency and therefore of greater apparent richness in stars. If one of the groups, e.g., the smaller one, be conceived to have the form of a widely extended and relatively thin stratum, the space swept clear will have a similar form and will mark upon the sky, as seen from any point near the center of the group, a luminous great circle whose plane passes through the direction of relative motion of the two groups. The galaxy is such a great circle and I have elsewhere indicated[1] that many of its features, not otherwise explained, result immediately from this conception of its nature. It is, however, fundamental to such a concept of the Milky Way that the absorption of star light should here be a minimum and the concept loses in probability if

[1] *Popular Astronomy*, **17**, 339, 1909.

such is not the case, although the star stratum would of itself tend to produce the appearance of a galactic ring, even though no absorption effect were present in sensible amount.

Directly pertinent to this matter is Kapteyn's extremely interesting research on "The Absorption of Light in Space," appearing in the *Astrophysical Journal* for November 1909, in which (**30**, 306) it is set forth that: "There is thus no reason for the supposition that the selective loss of light is different for galactic and extra-galactic regions. In particular we cannot explain the greater richness in stars of the Milky Way by a smaller space absorption for, if anything, this absorption is greater there than elsewhere." This result is obtained from a discussion of the discordances between the Harvard star magnitudes photographically and visually derived, and rests upon the assumption that if there be a sensible space absorption its effect must be apparent in a difference between the photographic and visual magnitudes, which difference will systematically increase with increasing distance of the stars observed. Classifying his material with respect to the source from which it is derived (Miss Cannon, Miss Maury), with respect to type of spectrum and to position near to or remote from the galaxy, Kapteyn makes a considerable number of determinations of the value of the coefficient d that serves as an index to the amount of absorption suffered by star light in transmission. In the mean these furnish a slightly greater value of d for galactic than for extra-galactic stars and this disparity is the basis for the conclusion above quoted.

To this treatment of the data it may be objected that if the galaxy is a stratum relatively free from absorbing matter, evidence of its character in this respect can be found only by comparing stars lying within the stratum with those outside it. Galactic latitude, which is the criterion used by Kapteyn to discriminate between galactic and extra-galactic stars, suffices for this purpose only when the stars are known to be very remote, and this condition does not generally obtain in Kapteyn's data since they relate for the most part to lucid stars. Thus, an average fifth-magnitude star situated in any galactic latitude may, and in many cases will, lie within the galactic stratum itself and its light will suffer as little absorption as does that of a star in the median line of the galaxy. Whatever view may be enter-

tained as to the nature of the galaxy, it seems evident that the effects of absorption of light will be more readily apparent in the case of distant stars than in those nearer to the earth, and in order that due weight may be given to this consideration, I have arranged Kapteyn's results in the following table, in which the first column shows the authority for the data, the second the spectral type of the stars employed, the third the mean magnitude of the stars, the fourth and fifth the mean centennial proper motion of the extra-galactic and galactic stars respectively, and the sixth the difference between the absorption coefficient found by Kapteyn for galactic and for extra-galactic stars of the given spectral type. These differences are so taken that a positive sign indicates an excess of absorption outside the galaxy. The last column shows the relative weights of the several determinations of $d_e - d_g$ computed from Kapteyn's weights for the individual quantities, depending chiefly upon the number of stars included in each group.

Authority	Spectrum	m	μe	μg	$d_e - d_g$	p
C	B_3	5.1	2″.2	2″.0	+0.0052	1.6
C	B_5	5.0	2.5	2.3	+ 29	0.9
C	B_8	5.2	3.1	2.6	+ 131	2.7
C	B_9	5.3	3.6	2.4	+ 10	1.0
M	A	4.3	7.6	4.5	+ 16	1.5
C	A	5.2	5.7	4.6	+ 35	7.5
M	A	4.9	9.2	5.9	+ 126	1.2
C	A_2	5.4	6.2	4.6	− 5	8.2
M	A_2	4.7	10.3	5.4	+ 28	0.6
C	A_3	5.0	6.8	6.0	− 447	1.2
C	A_5	5.0	9.8	6.6	+ 30	2.5
C	F	5.2	14.3	8.4	+ 292	1.3
C	G_5	5.0	34.7	30.5	− 98	2.7
C	K	5.0	13.3	11.9	− 132*	13.9
M	K	4.2	17.8	22.3	− 36	3.3

* Kapteyn questions the legitimacy of the data upon which this quantity depends.

If the average proper motions, μ, can be considered as an index of relative distance, the beginning of the table corresponds to remote stars, e.g., from five to ten times as distant as those represented at the end of the table, and contains therefore that part of the data best adapted to a determination of the quantity in question. This fact is not taken into account in the weights, p, assigned to the several results, and in fact Kapteyn, in deriving mean values of d, arbitrarily

diminishes the weight assigned to the quantities at the head of the table, thus in part rejecting the best evidence and relying upon the worst. This appears inadmissible, and taking the data as they stand, they seem better interpreted as follows: Wherever the material is at all well adapted to show a possibly existing difference between galactic and extra-galactic absorption (e.g., the first half of the table), it consistently shows by the sequence of plus signs an excess of absorption outside the galaxy. Where it is less well adapted to the end in view (e.g., the last half of the table), the results are conflicting and subject to such wide accidental variation that little reliance can be placed upon their mean value.

One might expect, a priori, that the results obtained would present this general character in respect of internal consistency, if the absorption outside the galactic stratum were really greater than within it, but the unbroken sequence of positive values of this difference presented by the more distant stars appears somewhat remarkable in view of the character of the data employed. To adopt this sequence of values as substantial proof of a diminished absorption of light within the galaxy is perhaps premature, but to ignore it and to draw the converse conclusion from the discordant testimony of the nearer stars is surely even less warranted. Whatever probative force the data may possess tends away from rather than toward Kapteyn's conclusions above quoted, and appears to render that conclusion entirely untenable, so far, at least, as the present data are concerned.

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February 4, 1910

REVIEWS

Temperaturbestimmung von 109 helleren Sternen aus spectralphotometrischen Beobachtungen. Von J. WILSING und J. SCHEINER. Publikationen des Astrophysikalischen Observatoriums zu Potsdam, Nr. 56, Bd. XIX, I. Leipzig: W. Engelmann, 1909. M. 15.

This notable publication includes spectral-photometric measurements of the sun and of 109 stars of magnitudes 4.2 and brighter, classified as of Vogel's types Ia1, Ia2, Ia3–IIa, Ib, IIa, IIa–IIIa, IIIa. The stellar spectra were compared in brightness at wave-lengths 0.448, 0.480, 0.513, 0.584, and 0.638 μ with the spectrum of an electric glow-lamp, itself compared with the "black" radiation of an electrically heated Heraeus oven of about 1500° absolute temperature. Assuming the applicability to the stars of Planck's formula for the distribution of radiation in the spectrum of a "black" body, the authors have computed from these data the temperatures of the sun and stars investigated.

Before discussing the results a few words may be devoted to the method of observation. A spectrometer with a flint glass prism was attached to the 80-cm refractor, thus giving a dispersion of 2°7 between $H\alpha$ and $H\gamma$. A corrector lens was used to reduce the chromatic aberration of the less refrangible rays. The stellar spectra were not broadened out for surface photometry, but diaphragms were introduced so as to select patches of spectrum at the desired wave-length, and these were treated as if they were stars to be compared in brightness by a Zöllner photometer with similar starlike patches of comparison spectrum. The latter spectrum was adjusted to equality in brightness with the stellar spectrum by means of a fixed and a rotating Nicol prism. For the brighter stars it was necessary to use means of reducing the intensity of the stellar spectrum. Numerous corrections were applied, among others for the absorption and reflection of the objective and corrector lenses; for the variation of focus; for atmospheric extinction as determined by G. Müller; for the current-strength in the glow-lamp; and for personal equation of the observers. The authors decide that the probable error for each wave-length of a final result of a spectrum comparison between a star and the glow-lamp, obtained as the mean of four observations by two observers on two separate nights, is

5.7 per cent. It is an interesting thing that there should have been no difference in probable error for the different wave-lengths, considering the great difference in sensitiveness of the eye in different parts of the spectrum and also the wide range of ratios of brightness in the stellar spectra. Numerous comparisons of the spectrum of the glow-lamp with that of the "black" body at determined temperatures showed a satisfactory degree of constancy of the glow-lamp, and yielded the relative intensities of its spectrum for the five wave-lengths observed, within a probable error estimated as not much exceeding 2.4 per cent. Hence, according to the conclusions of the authors, the probable error of a final determination of the brightness of a stellar spectrum for a given wave-length relative to its brightness at other wave-lengths would be about 6.2 per cent. In their observations of the sun the refractor was not used, but it was assumed in accordance with unpublished results that the sunlight reflected from a surface of chalk was unchanged in quality. Probably on account of the larger number of measurements the solar determination should be regarded as of somewhat greater accuracy than the others. By means of the spectral distributions determined by the five observed values for each star, the authors computed according to least squares by Planck's radiation formula the temperature of a "black" body nearest approximately the spectral distribution. They early found that the deviations of observed and computed distributions had a systematic course dependent on wave-length and not on temperature. This led them to investigate the spectral distribution in several terrestrial sources which might be regarded as approximately "black," and they found similar discrepancies. Accordingly they determined from the stellar observations themselves an empirical correction "Δ" of the following magnitudes for the several wave-lengths:

Wave-Length	$0\overset{\mu}{.}448$	$0\overset{\mu}{.}480$	$0\overset{\mu}{.}513$	$0\overset{\mu}{.}584$	$0\overset{\mu}{.}638$
Δ in percentages	+7.9	+0.7	−16.7	+2.8	+4.5

In the principal table beginning on p. 48 they give in column 3 the logarithms representing the directly observed spectral distribution, and in column 4 the logarithms corrected by the empirical quantity Δ. It is a great pity that the source of this supposed systematic error was not discovered, and its magnitude determined by laboratory experiments; for in the process of correction which involves the assumption that the spectra of the stars can be represented by Planck's formula, with temperatures of course determined from the same observations as the corrections, there is reasoning in a circle.

It is of interest to compare their mean result for the spectral distribution of the sun (S_λ) with that recently obtained by Abbot and Fowle. Unfortunately Wilsing and Scheiner do not give directly the solar spectrum intensities, but only their ratios (α_λ) to the intensity of the spectrum of glow-lamp No. 2 (E_λ) (glow-lamp No. 1, which was used for the stellar work, burned out before the solar work was done). From their statements on pp. 36, 38, 47, and 62, I take the following as the logarithms of the quantities to use to obtain the solar distribution:

Wave-Length	0.448	0.480	0.513	0.584	0.638
Log $\left(\alpha_\lambda = \dfrac{S_\lambda \text{ sun}}{E_\lambda \text{ lamp}}\right)$....	0.696	0.398	0.081	9.577	9.251
Log E_λ (No. 2)..........	9.269	9.623	9.889	0.457	0.777
Log Δ_λ................	9.967	9.997	0.067	9.988	9.981
I: Log $\alpha_\lambda E_\lambda$..........	9.965	0.021	9.970	0.034	0.028
II: Log. $\alpha_\lambda E_\lambda \Delta_\lambda$.......	9.932	0.018	0.037	0.022	0.009

On another hypothesis which the authors themselves tentatively adopted (see p. 45), I have considered the values on p. 38 from the standpoint that the work at $\lambda = 0.513$ should be neglected. On this basis I have made two calculations, first, adopting the values just given for E_λ; and second, assuming that glow-lamp No. 2 was in fact just like glow-lamp No. 1 (see p. 33), but for some reason its comparison with the "black" body was in error. On this basis I find:

Wave-Length	0.448	0.480	0.584	0.638
Log α_λ............................	0.696	0.398	9.577	9.251
Log E_λ (No. 2).....................	9.269	9.623	0.457	0.777
Log E'_λ (No. 1).....................	9.347	9.652	0.414	0.707
III: Log $\alpha_\lambda E_\lambda$	0.965	0.021	0.034	0.028
IV: Log $\alpha_\lambda E'_\lambda$	0.043	0.050	9.991	9.958

The work of Abbot and Fowle I take from unpublished spectrobolometric experiments at Mt. Wilson and Mt. Whitney of 1909, made with an especial view to determine the form of the solar energy-curve outside the atmosphere. In this work the scale of galvanometer deflections was uniform, and the corrections for change of sensitiveness of the bolometric apparatus, formerly so troublesome, were now almost entirely overcome. Various different optical arrangements were used, sometimes including five silvered mirrors and a flint glass or ultra-violet glass prism and sometimes with only two magnalium mirrors (no coelostat) and a quartz prism. The results are reduced to equality with each of the four Wilsing and Scheiner values at wave-length 0.448 μ as follows:

Wave-Length	0.448	0.480	0.513	0.584	0.638
W. and S. I...........	1000	1138	1012	1172	1156
W. and S. II..........	1000	1219	1274	1230	1194
W. and S. III..........	1000	1138	....	1172	1156
W. and S. IV..........	1000	1016	....	887	822
A. and F................	1000	1040	1000	893	800

We may suppose that Wilsing and Scheiner have employed substantially what is designated above as "W. and S. II" in their computation of the solar temperature, and they find $T = 5130° \pm 106°$ Absolute. This temperature, according to Wien's displacement law, would give a maximum of energy at $0.571\ \mu$. Abbot and Fowle's mean observed solar curve of 1909 gives its maximum at $0.460\ \mu$. It is clear that if the reviewer is correct in his understanding of Wilsing and Scheiner's solar work, there is a large discrepancy between their results and those of Abbot and Fowle, which, to be sure, would be sufficiently removed if the assumptions embodied in "W. and S. IV" above could be justified. If this last alternative could be accepted, whatever weight Abbot and Fowle's determination may have would go to confirm the accuracy of Wilsing and Scheiner's stellar observations, which were all made with glow-lamp No. 1. Otherwise, we must suppose one pair of observers or the other to be greatly in error.

To the reviewer, the reduction of the stellar spectral results to "black" body temperatures seems a by-product, rather than a principal result of the investigation worthy to have its place in the title; for, in the first place, it seems misleading to compute temperatures from a spectral range of only $0.2\ \mu$, whose distribution is fixed by five observations with probable errors of 6 per cent. each. Moreover, Planck's "black" body formula does not represent the distribution, even of the solar radiation, in all parts of the spectrum; nor ought it to be expected to, because the sun's spectrum is a composite spectrum: (1) the apparent temperature of the photosphere, as indicated by its energy spectrum, is wholly different at the limb from what it is at the center; (2) the apparent temperature at the center is probably a mean of a range of temperatures obtaining at different depths; (3) the violet end of the spectrum, from whatever part of the photosphere, is disproportionately weakened by selective absorption. It seems, therefore, of doubtful value to compute the temperature of the stars, in which these uncertainties are doubly uncertain, though possibly it might be worth while to make such computations as a mere curiosity. The values derived by many observers for solar temperatures seem chiefly interesting as they indicate the improbability of solids or liquids in the photosphere.

The main and highly valuable results of Wilsing and Scheiner's work, to

the reviewer, is contained in column 3 of the table beginning on p. 48. Its value would be powerfully enhanced if the uncertainties represented by the empirical correction and by the discrepancy between Wilsing and Scheiner and Abbot and Fowle could be removed. One regrets that *Capella* was not observed, because its spectrum is almost identical with that of the sun. For the following summary, the reviewer has ventured to omit the values at $\lambda = 0.513\,\mu$, and has computed the mean spectral distributions from column 3 (pp. 48 to 62) for four stars of each of the seven spectral classes investigated.

Type	Stars		Intensity			
			$\lambda\ 0^{\mu}.488$	$\lambda\ 0^{\mu}.480$	$\lambda\ 0^{\mu}.584$	$\lambda\ 0^{\mu}.638$
Ia1	*β Can. Min.*, *α Delphini*,	*12 Can. Ven.* *α Pegasi*	1000	836	579	505
Ia2	*α Androm.*, *γ Ophiuchi*,	*γ Coronae* *γ Lyrae*	1000	796	625	525
Ia3–IIa	*α Trianguli*, *δ Leonis*,	*ξ Geminorum* *δ Aquilae*	1000	948	902	845
Ib	*γ Pegasi*, *ρ Leonis*,	*η Leonis* *ζ Pegasi*	1000	887	578	530
IIa	*η Boötis*, *μ Herculis*,	*β Virginis* *γ Cygni*	1000	998	993	1005
IIa–IIIa	*α Arietis*, *δ Cancri*,	*ο Tauri* *β Ophiuchi*	1000	1205	1766	1897
IIIa	*α Orionis*, *χ Serpentis*,	*δ Virginis* *δ Sagittae*	1000	1368	3296	4406

Whatever may be the justification for the correction called by the authors Δ, or the uncertainty raised by the discrepant solar values, the combined magnitude of these possible systematic errors as compared with the changes between different spectral types as shown in the table just given is too small to nullify the very substantial value of the distinctions which this great piece of work now enables us to make in the spectral energy distribution for the various stars investigated.

C. G. ABBOT

Les observations méridiennes. Par F. BOGUET. Tome I, "Instruments et méthodes d'observation," pp. 314; figs. 96; Tome II, "Corrections instrumentales et équations personnelles," pp. 342; figs. 76. Paris: Octave Doin et Fils, 1909. Fr. 10.

These two volumes form part of the "Bibliothèque d'astronomie et de physique céleste" of the *Encyclopédie scientifique*, which is being publishedunder the direction of Dr. Toulouse.

The treatment of the subject is both theoretical and practical, and is adapted to the needs of the amateur as well as the professional astronomer.

Tome I deals with the fundamental conceptions of the celestial sphere, meridian instruments and their accessories, clocks, chronographs, and the methods of making observations for the determination of time, right ascension, and declination.

Tome II is devoted to a consideration of the errors which may affect observations made with meridian instruments.

For the professional astronomer this second volume is by far the more important. The meridian observer is no longer satisfied with a precision of one-tenth of a second of arc. He wants to be sure of the hundredth of a second, and this degree of accuracy can be attained only by taking into consideration every known error. It is fitting therefore that a whole volume should be devoted to this subject. Some idea of the detail with which the various errors are discussed may be obtained from the fact that the errors of adjustment cover 115 pages; errors of construction, 95 pages; refraction, 26 pages; and personal equation, 67 pages. No numerical illustrations of any kind are given.

The work is thoroughly up to date. Throughout the text are numerous references to original sources, and, at the end of Tome II, is a 22-page bibliography.

FREDERICK SLOCUM

Spectroscopie astronomique. Par P. SALET. Paris: Octave Doin et Fils, 1909. 12mo, pp. 425, with 44 figures and 1 plate. Fr. 5.

This handy volume is one of the units of a very extensive *Encyclopédie scientifique*, to be comprised in about one thousand volumes. Twenty-nine are to form the subdivision entitled "Bibliothèque d'astronomie et de physique céleste," of which M. J. Mascart, of the Observatory of Paris, is "directeur."

Following an excellent general introduction, the subject is compactly treated in thirteen chapters. The first three chapters consider the instruments of spectroscopy and their adjustment, the fourth discusses measures and standards of wave-lengths, the fifth treats of the physical causes affecting the appearance or position of spectral rays, while the sixth is devoted to the Doppler-Fizeau principle. The remaining chapters have for topics the solar spectrum, and the spectra of the different kinds of celestial objects —sun, planets, comets, stars, and nebulae.

The treatment is generally clear and succinct, with some reference to the historical development in each line. There are no marginal references,

but at the end of each chapter a brief bibliography is given, which is particularly adapted to the convenience of the French readers of the encyclopedia. Recent researches are not neglected, so that the work is up to date. This is important, for there are now hardly any books, in any language, available in the field of celestial spectroscopy which include the progress of the last decade.

The defects of the book are those rather unavoidable in any single part of such an immense cyclopedia: the paper is not as good as might be desired, and many of the illustrations are exceedingly unsatisfactory. Improvement in these respects would, however, have increased the cost—a very significant item for the whole series of books. But it might have been as well to omit some of the reproductions of spectra as to print such inadequate ones.

The author has selected his material from reliable sources, and, as is natural and proper in such a work, gives full prominence to the researches of his countrymen. We note, on page 150, that he accepts as real the large differences between wave-lengths in arc and spark found by Haschek and Mache, but not confirmed by other spectroscopists.

The book is to be commended to astronomers and physicists—astrophysicists will secure it as a matter of course—and we shall await with interest the appearance of the other volumes of the series relating to astronomy and to physics.

E. B. F.

Annuaire pour l'an 1910 publié par le bureau des longitudes. Paris: Gauthier Villars, 1909. Pp. 862. Fr. 1.50.

This valuable annual returns in its usual form. With the scheme of alternation in effect since 1904, the volume for 1910 gives detailed tables of a physical and chemical nature, but omits those concerned with geography and statistics. Similarly, this year the elements of the minor planets are tabulated in full, while the lists of stellar parallaxes, double stars, etc., given last year, are omitted. We note an error on p. 228 in the assignment of Des Moines instead of Williams Bay as the discovery point of Comet 1908 III (Morehouse).

The "notices" appended to the *Annuaire* are: a note upon the meeting in 1909 of the permanent international committee of the astrographic chart, by M. B. Baillaud; an essay of ninety pages on the tides of the earth's crust and the elasticity of the globe, by M. Ch. Lellemand; and indices of the notices published in previous issues of the *Annuaire* from 1804 onward, by M. G. Bigourdan.